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Psychological Bulletin

THE SENSORY CAPACITIES OF INFRAHUMAN MAMMALS: VISION, AUDITION, GUSTATION

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I. Introduction

In spite of the voluminous literature on animals and animal experimentation, huge lacunae exist with respect to detailed and reliable information concerning their sensory capacities and limitations. R. L. Yerkes has pointed out this shortcoming with respect to the chimpanzee (**181**) and the other great apes (**185**).

N. L. Munn's 1933 text on animal psychology, which carried the subtitle, "The Behavior of the Rat" (**105**), devoted a scant thirty pages to the sensory processes of the animal that had been the subject of research for over a quarter of a century. In his recent monumental volume (**107**) reviewing psychological resarch on the rat to 1950, much more information concerning the sensory capacities of this animal is available, largely as a result of research since 1930. He points out (**107**, p. 180), however, that although in no other animal, with the possible exception of man, has sensitivity been as thoroughly investigated, large gaps in knowledge still exist with respect to color vision, hearing, and the limits of sensory differentiation in other modalities. The paucity of information available, however, can be appreciated only when the fragmentary facts are assembled and compared with the extent of the animal kingdom. It is the purpose of this review to collect the quantitative data with respect to the limits of visual, auditory, and gustatory capacities of infrahuman mammals, to indicate the coverage in terms of species and experimental populations, and to describe and compare methods employed to determine these capacities. The choice of topics and topic subdivisions has been largely dictated by the volume of data available. Vision and audition both represent areas of considerable work; gustation has been treated in a driblet of papers; and all other receptive capacities combined are relatively terra incognito.

General reviews may be found in the comparative psychology texts,

particularly Watson (**167**) to 1914, Washburn (**163**) to 1926, Maier and Schneirla (**99**) to 1935, Warden, Jenkins, and Warner (**160, 161**) to 1936, Fields (**42**) to 1946, and Harlow (**65**). Yerkes (**181**) and Yerkes and Yerkes (**185**) have summarized the data for chimpanzees and the great apes, and Ruch (**134**) has prepared a rather complete (to 1941) bibliography of the literature on primates. Munn (**105, 107**) summarized the data for the rat. Razran and Warden (**124**) reviewed the findings with respect to the sensory capacities of dogs as determined by Russian workers using the conditioned reflex method (up to 1929). Honzig (**73**) and Ghiselli and Henry (**49**) briefly covered the role of sensory acuity and the sensory thresholds of rats in relation to maze learning. Annual reviews of current findings relating to sensory processes are given by Davis (**29**), Hartline (**67**), and Olmstead (**112, 113**). These reviews contain brief references to studies of mammalian sensory capacities.

There seems to be little question concerning the psychological importance of investigations of the sensory capacities of numerous mammalian species. The animal is well established as a research tool to supplement work on man and to provide information that cannot be obtained with human subjects. It should be stressed, however, that no transfer from infrahuman to human behavior can be made with confidence until contingent similarities and differences in sensory capacity are determined and evaluated. The selection of animals for particular studies, the development of appropriate apparatus, and the formulation of suitable experimental designs depend, or perhaps *should* depend, upon the characteristics of the animals themselves. Fundamental among these characteristics are the limitations and response ranges of the sensory modalities through which stimuli are received. Unless the limits of these sensory modalities are known for large groups of animals, intelligent selection of experimental populations and adequate experimental designs are difficult or impossible.

II. Vision

More has been written about vision in mammals than about all the other sensory modalities combined.

The field of vision has traditionally been broken down into a variety of subfields: visual acuity, brightness discrimination, color vision, movement discrimination, flicker discrimination, depth discrimination, form and pattern discrimination, size discrimination, and distance discrimination. In the majority of these areas, however, substantial data concerning threshold functions are lacking. For example, various investiga-

tors have been able to demonstrate brightness discrimination behavior in mice, rats, guinea pigs, rabbits, chickens, raccoons, porcupines, cattle, dogs, monkeys, cats, and horses. But in spite of the fact that this is one of the easiest discriminations to establish, variations in apparatus and methods have made it almost impossible to state comparable threshold values. Form discrimination has also thus far not yielded to adequate standardization which would permit cross-species comparisons. This paper will therefore limit consideration to studies dealing with the measurement of visual acuity thresholds and color vision, with incidental reference to the role of brightness in color response behavior.

In addition to the general treatments referred to in the introductory paragraphs, vision in mammals (anatomy, physiology, and visual capacities) is treated in Bartley (**8**, **8a**), Detwiler (**30**), Granit (**53a**), Parsons (color vision) (**115**), Rochon-Duvigneaud (**131**), Stagner (**145**), and Weymouth (**179**). Detwiler's *Vertebrate Photoreceptors* and Bartley's *Vision* are especially valuable for their accounts of the visual mechanism. However, the data on the response capacities of mammalian species are conspicuous by their absence. The visual literature has been catalogued by Fulton, Hoff, and Perkins (**44**) in a bibliography of over 3,400 items.

Visual Acuity

Redetermination of the visual threshold of man by Hecht, Schlaer, and Pirenne under the "most favorable conditions of dark adaptation, peripheral vision, small test fields, short exposure, and selected portions of the retina" yielded an absolute threshold of 2.1 to 5.7×10^{-10} ergs in the blue-green, corresponding to between 54 and 148 quanta at the cornea, and probably 5 to 14 quanta absorbed by the rods, or an absorption of about 1 quantum each by 5 to 14 rods out of 500 in the geometrical image of the stimulus. With so few quanta involved, human thresholds should (and do) show great variation. It is to be anticipated that similar variation, based upon the same considerations, should appear in other species.

Minimum separable and minimum visible thresholds. Minimum separable acuity (the acuity of distinguishing two lines in close apposition) and minimum visible acuity (the acuity of distinguishing a single line against a homogeneous background) have been regarded as dependent upon the same processes, but the thresholds for the two functions vary greatly. In man, minimum separable acuity varies between 1.0 and 0.5 minutes of arc, while minimum visible acuity approximates 0.066 minute. With the exception of Klüver (**90**), Warkentin (**162**), and

Smith (142), however, determinations in animals have been in terms of minimum separable acuity only (see Table I).

Methods. The discrimination technique has constituted the primary method employed in the study of visual acuity in animals. Some modification of the Yerkes-Watson discrimination box (184) has been used by Spence (144) and Johnson (84, 85, 86, 87). Lashley employed the jumping technique he devised for the study of pattern vision (95, 96). Smith (141, 142) employed a combined discrimination and conditioning technique, which seems to be a version of the Yerkes-Watson apparatus in which an electric shock punishment is introduced before the animal reaches a goal point. A stimulus box is set at the end of each of two runways. After release from the cage, the animal is required to make a discrimination response at the end of the runway table away from the boxes. If the response is correct, the animal may approach the box and obtain the food by pressing a lever. If the response is incorrect, it is shocked at the beginning of the runway.

In the discrimination studies, the stimuli characteristically are modified Ives instruments which present the animal with visible striae. One test field is adjusted to a point where it appears homogeneous, and in the other the striae are gradually reduced until a discrimination between a striped and homogeneous field breaks down. Another method employed involves setting up the fields so that the striae in one field are at right angles to the striae in the other. The separations are then reduced until this discrimination breaks down. Johnson (85) found that the two methods yielded comparable results.

Warkentin (162) determined visual acuity thresholds by means of oculocephalogyric responses to a moving visual field. The animal was placed in a rotating cylinder that contained vertical black and white striations which took the place of the Ives gratings. Smith and Warkentin (143) employed optokinetic responses in a similar manner.

Klüver (90, p. 227 ff.) employed an unlearned response (grasping) technique in his work with monkeys. The animal is confronted with a box near the top of which is an opal glass, illuminated from the bottom. Within reach of the animal, and lying on the glass, is a thread to which food is attached. If the thread is visible, the animal grasps it and pulls in the food. The experiment is performed in a dark room with illumination available only from behind the opal glass. Klüver also used the "pulling-in" technique in a variation of the traditional discrimination experiment. The animal faced two or more boxes, above one of which was placed a positive stimulus field (e.g., a cardboard with a line drawn on it) while above the others a blank card was placed. The

animal was trained to pull toward him the box under the positive stimulus field.

While the results obtained from this last method may provide a rough index of acuity or discriminative capacity, the method as used is probably too uncontrolled in terms of brightness, accommodation distance, and especially measurement to yield very exact threshold values.

Results of visual acuity studies. Visual acuity has been demonstrated to be a function of the test object, accommodation distance, general field illumination, and illumination of remote regions (8, p. 34 ff.), when stimulus characteristics are considered. These are quite probably the minimum variables from the stimulus side. Yet the reported animal studies do not specify all these factors, nor are sufficient data available to reduce to a common denominator acuity measurements resulting from different test situations. Table I presents visual acuity figures found in the literature for mammalian species.

It is to be noted that wide interspecies differences manifest themselves. However, in view of the extremely limited character of the samples employed, it may be that intraspecies variation is still greater, except for the most divergent species.

Visual acuity and other visual functions. Visual acuity, as distinguished from brightness, size, distance, and form discrimination, shows little relationship with phylogenetic status (162), although from a structural standpoint great interspecies differences may be noted with respect to the fineness and distance apart of the receiving elements and in the number of conducting and association elements. It is therefore possible that phylogenetic differences with respect to discriminations other than acuity are to be accounted for, not in terms of any improvement on the optical efficiency of the eye, but in perceptual processes associated with vision and in the intelligence of the animals in solving the problems by means of which an experimenter measures brightness (or other) discrimination. On the other hand, precisely these interspecies structural differences may be invoked to account for visual acuity differences. Grether (63) reviewed the data on minimum separable acuity thresholds for the chimpanzee, rhesus monkey, Cebus monkey, rat, pigeon, and gamecock, and used the data for a computation in terms of retinal image width. He concluded that thresholds thus expressed provide a more direct basis for interspecies comparison of ocular efficiency than do visual angles. Because of their smaller eyes, all animals considered compared favorably with man when retinal widths were substituted for visual angles as a measure of acuity.

TABLE I

VISUAL ACUITY THRESHOLDS IN INFRAHUMAN MAMMALS*

Animal	*N*	*Method*	*Accommodation Distance* (cm.)	*Brightness* (mll.)	*Minimum Separable Acuity* (min.)	*Minimum Visible Acuity* (min.)
Rhesus monkey (171)	2	Discrimination box	72	37.0 0.75	0.90 0.67	
Chimpanzee (144)	2	Discrimination box	120	3.98	0.47	
Cebus monkey (85)		Discrimination box	60	3.85	0.95	
Java monkey (90)	1	Thread-grasping ("pulling-in" technique)	28	(0.007)†		2.5
Cebus monkey (90)	1	Discrimination—single thread ("pulling-in" technique)	200	Sunlight		0.044
Albino rat (96)	3	Jumping technique	20		52–86	
Pigmented rat (96)	5	Jumping technique	20		26–52	
Cat (142)	2	Discrimination and conditioning	50–120		5.5‡	0.45–3.3
Domestic rabbit (162)		Oculocephalogyric response				1.4–1.8
Wild rabbit (162)		Oculocephalogyric response				0.50
Guinea pig (162)		Oculocephalogyric response				.50–1.4

* All studies but Warkentin's (162) used visual striae produced by some version of the Ives apparatus.
† Foot-candle illumination.
‡ Not a threshold, but the minimum obtained with the apparatus.

The precise meaning of this conclusion for comparative studies is not clear, however. A four-cylinder gasoline engine will yield more miles per gallon than an eight-cylinder engine. If a comparison is to be based upon gasoline consumption, the smaller engine is "better." However, if other performance characteristics such as acceleration, speed, and hill-climbing ability are taken as criteria, the larger engine invariably proves the "better." If interspecies comparisons are to be made in terms of the dimensional characteristics of the "real world," then the latter kind of analysis is assumed. An interesting experimental problem is posed, however: What is the relationship between acuity of sensory discrimination and adequacy of perceptual and cognitive organization? Ghiselli and Henry (**49**) found a correlation of .31 between differential proprioceptive sensorv thresholds and learning in rats, and one of .36 between sound thresholds and learning, when the stimuli were above the absolue threshold. Too frequently, however, the absolute threshold is not known or considered. For example, if the rat's eye is as efficient as the human eye when the physical stimulus components are equated in terms of comparable retinal image width, then under these conditions what are the characteristics of rat learning? It is suggested that learning experiments might be arranged to take these factors into account. Typical apparatus design calls for scaling equipment down to the size of the animal, whereas these considerations suggest that the size of the test object must be increased as retinal image width decreases if the level of difficulty from point of view of basic sensory discrimination is to be held constant.

It seems clear that the interrelationships of these factors have not yet been adequately explored, and that the data essential to undertaking such investigations are still lacking. In this connection, Harlow (**65**) points out that no discussion of visual discrimination is complete without noting the great emphasis placed on utilizing efficient apparatus, in contrast to the comparatively slight emphasis placed on utilizing efficient stimuli. Kohts (**183**), for example, found that stereometric objects are more easily discriminated by the chimpanzee than are the types of stimuli (designs painted on flat surfaces) commonly used in animal laboratories, and it is not unlikely that this finding would hold good for many other species.

Color Vision

One of the most controversial topics in animal psychology has been that of color vision in species below man. In the first place early experimental evidence suggested it was lacking in such favorite laboratory

animals as the rat. In the second place, theories of vision that consider color discrimination dependent upon an evolutionary process in the eye (and color blindness in man an evidence of atavism) seem to require that color vision, if it exists below the primate level, be very rudimentary at best (**58**). The doctrine that color vision was probably lacking below the primate level in mammals therefore became an accepted part of the literature, and led Munn to conclude that " . . . the rat is color-blind . . . [and] these results . . . are amply substantiated by many other experiments involving infra-primate mammals, all of which appear to lack cones" (**105**, p. 131).

The more recent evidence has reversed or substantially qualified this conclusion, although Munn, in his latest summary of the issue (**107**, p. 143), suggests that the existence of color discrimination in the rat is still so controversial a question that further research to refute or support the available positive results is required.

Three lines of evidence are available for the study of mammalian color vision: first, experiments in which the animal is trained to make a positive response to one color and no response or a negative response to another; second, anatomical and histological studies of the eye to determine the presence or absence of cones; and third, electroretinographic studies to determine by comparison of the spectral curve in the light-adapted eye with the curve of dark adaptation, whether the differences characteristic at the human level appear (Purkinje phenomenon, displacement of photopic curve).

This paper will be limited to the first line of evidence primarily, with reference to a few of the findings concerning the spectral curves of some species.

Methods. A wide variety of techniques have been employed to determine the existence of color vision in mammals. The most frequently used apparatus has been the Yerkes-Watson discrimination box or some modification of it (**21**, **89**, **103**, **104**, **106**, **108**, **155**, **157**, **164**, **168**). Walton and Bornemeier (**158**) and Russell and Walton (**135**) worked with the Lashley jumping apparatus. The conditioned response technique was used in Pavlov's laboratory (**120**), by Walker in Bekterev's laboratory (**124**), and by Brown (**18**). Klüver had monkeys pull in strings to which differently colored boxes were attached (**90**).

As wide a range of light sources as techniques have been utilized. The most precise light source, spectral colors, was used early by Watson (**166**, **168**) and more recently by others (**18**, **53**, **59**, **75**, **149**). More frequently, however, monochromatic light was obtained by means of Wratten or other filters (**103**, **106**, **108**, **155**, **157**, **158**). Hering and

Bradley papers (**10, 75, 89, 104, 164, 169**), dyes (**139**), paints (**21, 22**), colored disks (**55**), cards (**19**), and the color wheel (**92**) also have provided the color stimuli.

This variety in method has without doubt contributed to the lack, or apparent lack, of consistency in results, particularly in the case of animals with which a large number of experiments have been performed. The most serious shortcoming, one which seems to be common to almost all the earlier experiments, is the absence of adequate control of the brightness factor (**145**).

In the first place, adequate brightness control is difficult or impossible when papers or paints are used, and presents some problem with filtered lights that are not strictly monochromatic. In the second place, in some instances brightness of the stimulus colors was matched for the human eye. Infraprimate mammals generally react much more readily to brightness differences than to hue differences, and it has been demonstrated that hues matched for brightness to the human eye may appear of different brightness to the animal's eye. Current technique therefore calls for the selection of a pair of hues demonstrably equivalent in brightness for the experimental animal, and subsequent training of different animals from those on such a pair of colors used for the selection ("confusion colors"). Finally, after differential discrimination to this pair is established, control experiments in which the brightness relations of the colors are systematically varied are necessary.

Experimental evidence on the presence or absence of color vision is available for only a very few species, and conclusions in which any confidence can be placed are possible only for primates, rats, and possibly dogs.

Squirrel and porcupine. Sackett (**136**) reported positive findings with respect to the existence of color discrimination in the porcupine, and Colvin and Burford (**22**) claimed evidence that the squirrel could discriminate colors, but in neither case was brightness controlled.

Raccoon. Cole (**19**) and Cole and Long (**20**) reported positive results, but brightness was not controlled, or it was merely equated for the human eye. Davis (**28**) found that his animals were responding to a brightness difference, rather than a hue difference. Gregg and McPheeters (**56**) obtained some evidence of color discrimination, but in a later study Gregg, Jamison, Wilkie, and Radinsky (**55**) found that the animal could not discriminate pairs of colored disks.

Calf. Kittredge (**89**) trained a calf to discriminate red from a Hering No. 5 gray, but the animal could not discriminate the red from a No. 50, suggesting that it was responding to a brightness difference.

Dog. Colvin and Burford (**22**) trained three dogs to pick a positive

color out of nine colors, but brightness and experimental cues were not controlled. Samojleff and Pheophilaktowa (**137**) found that their dog could discriminate green from most grays, but that one or two of them were frequently confused. However, even these grays were discriminated from the green only after extended training. Smith (**140**), using transmitted light, found that some dogs apparently could make color discriminations, but he concluded that the discrimination was very unstable "and cannot be supposed to play any part in the animal's normal existence." Walker, in Bekterev's laboratory, was able to establish a conditioned color response only after 3,000 trials (**124**). Pavlov found that the salivary reflex could not be conditioned to a color stimulus (**120**, p. 101). Yerkes and Morgulis (**182**) similarly reported that Orbeli failed to establish a conditioned respose to color in several hundred trials. They concluded that "since this is the most delicate method which has probably ever been devised for studying differential sensitivity, it is not likely that the dog has color vision."

Guinea pig. Allen (**3**), employing a Yerkes apparatus, found no evidence of color discrimination in this animal.

Cat. Devoss and Ganson (**31**) found that cats could discriminate colors from gray papers, but not from gray cambric. Colvin and Burford (**22**) trained a cat to pick a positive color out of a group of nine colors, but brightness was not controlled; Gregg, Jamison, Wilkie, and Radinsky (**55**) failed to establish discrimination between three pairs of colors. Granit (**54**) determined the curve of photopic vision by placing electrodes on the retina. He found the point of maximum sensitivity at 560μ, and a spectral curve that approximated the human curve of photopic vision. This finding does not seem to be decisive, however, because Graham, Kemp, and Riggs (**52**) were unable to establish a cone vision curve in the pigeon, which does discriminate colors.

Rabbit. Washburn and Abbott (**164**) found that the rabbit could not discriminate between two doors leading to food when the stimuli to be differentiated were a red and a Hering No. 46 gray. However, Brown (**18**), using conditioned respiratory responses, determined the brightness visibility curve and the dim visibility curve of the rabbit. He found that the dim visibility curve was identical with the human dim visibility curve and the brightness curve was displaced 30μ toward the red end of the spectrum. Further, he demonstrated a consistent differentiation between red and green in spite of systematic variations in brightness.

Rat. The bulk of work on color vision below the primate level has been done with rats (and the writer will include mice). At least until 1933 the evidence has been interpreted to mean that these animals lacked color vision (**21**, **74**, **75**, **76**, **97**, **104**, **106**, **168**, **169**, **180**). Munn's (**105**) earlier comment in this connection has been cited above.

The anatomical evidence is not so clear-cut, however. Vincent (**151**) and Lashley (**97**) found no evidence of cones in the rat's eye, nor Hopkins (**74**, **75**) in mice. However, Detwiler (**30**, pp. 22–23) reported the meas-

urement of the cone layer in the eyes of various vertebrates. He listed positive measurements for the marmoset, rhesus monkey, and chimpanzee; indicated that accurate measurements were uncertain in the dog and cat because of pigment masking or unsatisfactory histological preparations; listed "very sparse or absent" for the rat and hamster; and stated that cones were lacking in the bat, lemur, and owl monkey. Walls (**153, 154**) found that rats *do* possess cones, but he is still skeptical of the results of recent studies indicating that the rat can make at least limited color discriminations. To complicate matters further, Graham and Riggs (**53**) recorded the retinal potentials from the eye of the rat in response to lights of various wave lengths. They found that the "retinal response of the rat's eye is one which varies only in relation to brightness; a response for any wave-length may be matched by a response for another wave-length provided the proper intensity is found for the second wave-length." Then, in a later study, Graham, Kemp, and Riggs (**52**) were unable to obtain a cone curve from the eye of the pigeon, which has color vision.

The history of color vision experiments with rats is given in detail in Munn (**107**, pp. 134–143). By 1933 the accumulated evidence suggested that the animal was color-blind. Then color discrimination was demonstrated by Walton (**155**); this study was criticized for inadequate control of brightness by Munn (**106**), who seemed to demonstrate that Walton's findings might in fact have been due to the brightness variable rather than the hue variable. Then, Muenzinger and Reynolds (**103**) obtained a discrimination on a discrimination box apparatus between red and green, and one between red and black. Their results were confirmed by Munn and Collins (**108**). Walton (**156**) and Walton and Bornemeier (**157, 158**) extended and perfected their work, using a discrimination box and a Lashley jumping apparatus. Discrimination between matched pairs of yellow-blue, red-green, and red-blue, somewhat unstable discrimination between red and yellow, and no discrimination between blue and green or between yellow and green were found. It was significant that while a brightness discrimination could be established in an average of 85 trials, a hue discrimination required between 375 and 525 trials. In some cases Walton and Bornemeier employed 800 trials to obtain positive results.

Most recently, Russell and Walton (**135**) demonstrated a red-blue discrimination in 18 rats, albino and hooded, subjected to avitaminosis-A. As the avitaminosis continued, accuracy of response was maintained at 95 to 99 per cent. These results indicate that avitaminosis-A, which has a marked effect on rod vision, may have no effect on cone vision. Munn (**107**, p. 142) suggests further that the failure of avitaminosis-A to disturb the red-blue discrimination may support the conclusion that the discrimination is based on hue rather than on small brightness differences.

Muenzinger and Reynolds concluded that rats possess a hierarchy

of discrimination habits consisting of a "primary brightness relation habit, a secondary absolute brightness habit, and a weak color discrimination habit." The relative brightness values of the colors have been established as follows: blue has the highest value for the rat, green and yellow come next, and red has the lowest value.

Infrahuman primates. Positive findings with respect to the existence of color vision were noted in all but one out of about a score of papers. Color vision has been demonstrated in the chimpanzee (**45, 59, 60, 61, 62, 64, 91, 92, 183**), the orangutan (**45**), and in the Java, Cebus, spider, and rhesus monkeys (**10, 58, 88, 90, 139, 147, 166**). It has been demonstrated with papers, the color wheel, painted disks, and spectral colors. It has repeatedly been shown that such discriminations are independent of brightness (**10, 58, 59, 60, 61, 64, 139, 183**).

With this array of evidence available, it becomes possible to review the specific characteristics of this capacity in infrahuman primates. Early research (**10, 88**) suggested that color discrimination is easier for these animals than brightness discrimination (discrimination among grays). However, this result may have been due to interference with previous training rather than an inherent limitation. For example, Bierens de Haan (**10**) presented his subjects (two pig-tailed macaques) with a five-choice apparatus. Each of five boxes had a color on one side: white, black, red, yellow, or blue. The animal was trained to respond to, say, the white. The boxes were systematically shifted about so that the white was distributed according to a chance sequence throughout the trials. After discrimination of the white from the other colors had been established, the other colors were replaced with grays, and the discrimination habit was found to persist. The same procedure was employed with each of the five colors as the positive stimulus. However, when a gray was made the stimulus color in a series of five grays, no discrimination habit could be formed. In addition to the possibility here that brightness was not adequately controlled, it seems likely that the second habit interfered with the first. Kuroda (**93**) repeated Bierens de Haan's experiment and obtained negative results, the only study the author has noted in which this is true. Kohts (**183**) trained a chimpanzee to select, from forty colored disks, one to match in hue a disk offered by the experimenter. She noted, however, that achromatic stimuli were not as well distinguished as chromatic stimuli.

The most intensive work with primates has been undertaken by Grether. His careful explorations of the limits and characteristics of primate color vision are models of experimental investigation. He used monochromatic lights with a discrimination apparatus in which the animal was required to take food out of a box on which the positive stimulus color was projected. In one experiment (**58**) with monkeys he determined the limens for hue discrimination as follows: the animal was trained to respond positively to yellow (589μ) and negatively to blue-green

(500μ). Training continued through changes in brightness relations until the color habit was fixed. Then the wave length of the negative color was increased 20μ, and in consecutive trials increased gradually until discrimination between the yellow and the negative color broke down. A criterion of 75 per cent correct in 25 trials was set, and each wave-length step was offered in 25 trials. The procedure was repeated to obtain limens for red and blue-green. Table II presents the limen values in μ for the animals tested. Except for the Cebus monkeys' response to the red and yellow, these limens are almost identical with the human limens.

TABLE II

COLOR VISION LIMENS IN MONKEYS IN MU
(From Grether [58])

Primate	*Stimulus Color*		
	Red (640μ)	*Yellow* (589μ)	*Blue-green* (500μ)
Cebus Monkey	30–45	4–8	7–10
Spider Monkey	6	2	10
Old World Monkeys	8–12	2–4	7–10
Man	10–12	1–3	6–21

Repetition of the experiment with chimpanzees (59) showed that chimpanzee limens for hue discrimination are approximately equal to that of man in the blue-green, but about twice that of normal human vision in the yellow and red. When response of chimpanzees in terms of color mixture proportions was compared to man's, it was found that chimpanzees required more red in a red–blue-green complementary mixture to match white, but that the proportions of red and green to make a yellow were the same for the two species. Using a color against darkness under conditions of light adaptation (15 seconds' exposure to a light before making the discrimination), Grether found the spectral limits of chimpanzee vision to be between 402μ and 704μ; for human beings the results were between 399μ and 692μ (61). The spectral saturation curves for chimpanzee color vision were determined by finding the proportion of spectral color mixed with white needed to produce a discriminable hue (64). The curves for two chimpanzees were very close to those for two human beings, the lowest saturation point for human subjects being at 575μ, and for chimpanzees 570μ (62). Grether summarizes his findings with the conclusion that the color vision of the chimpanzee and the rhesus monkey is at an evolutionary position just short of human vision, and that the slightly deficient hue discrimination of these animals could be accounted for by less complete differentiation of the

red and green receptor mechanisms. The markedly anomalous hue discrimination of the Cebus monkey is taken as evidence of two-color vision in that animal (**59**). The observation that the crossing point in red-green discrimination (the discrimination habit reverses as the difference becomes subliminal) is about 5μ nearer the shorter end of the spectrum in chimpanzee vision than in normal human vision, is taken to mean that chimpanzee vision slightly resembles red-green color blindness, another evidence of lesser development (**64**).

Grether's findings are highly provocative, not only for the information they provide about primate color vision but for the implications for color vision theory. It seems clear, however, that further corroboration is necessary, since his animal samples are very small (involving at most nine monkeys in one study). Two sorts of questions are pertinent: first, are the differences between infraprimate and human vision noted here within the sampling error, and within a broader range of variation among the animals? Second, to what extent has the laboratory environment, with its limitations and restrictions, interfered with the development of color vision habits in these animals? Are there differences between laboratory animals and animals in their natural setting in this regard?

Color vision may be regarded as an established fact in some primate species. In view of the evidence that owl monkeys lack cones (**30**, p. 23), this species may not be able to discriminate color. A beginning has been made in the determination of the limits and quality of primate color vision. The positive evidence in favor of color vision in rats seems clear, but it has not yet been reconciled in any adequate fashion with the electroretinographic findings. These perhaps may be reinterpreted or revised, or an essential independence between retinal response to spectral lights and color vision may obtain. It seems quite likely that, in a qualified way, the latter alternative will prove to be the more appropriate.

With respect to other mammals, the volume and character of the evidence fail to justify any positive conclusions. The history of experimentation with color vision in the rat should discourage ready acceptance of negative findings with other animals on the one hand, and on the other the failures in early experimentation to control contingent variables, particularly brightness, inspire little confidence in what meager positive findings have been made. Adequate exploration of the color sensitivity of mammals is still a task for the future.

III. Audition

Two interrelated experimental problems have been attacked with reference to the modality of hearing: First, can animals discriminate pure tones from noise, and one tone from another? Second, if they can discriminate tones, what are the characteristics of their audiograms?

It is not improbable that mammals differ considerably with regard to the capacity to discriminate sounds on the basis of wave-length differences (**161**, p. 293). The first problem, however, seems to have developed *as a problem* as much through an overzealous application of the law of parsimony as from the experimental data. Its history is reminiscent of the history of the color vision issue. Firm opinions were formed with respect to the guinea pig, the dog, the cat, and particularly the rat, to the general effect that these animals could not discriminate pure tones, or could discriminate them only poorly. It was not denied that they could hear, or that they could discriminate noise from silence. Until the mass of evidence was clear, however, it was assumed that all pure tones sounded alike, or that they were not adequate stimuli for the animals' auditory receptors. With the resolution of this problem in the conclusion that these animals *do* discriminate pure tones, attention was directed toward the second question. Two aspects of it will be explored in particular: the extent of the auditory frequency spectra in infrahuman mammals, and the minimal intensity values (relative to human thresholds) required to evoke a response at various frequencies. To a greater extent than in other sense modalities, the more recent results have been both clear and consistent for several infrahuman mammalian species.

Current summaries of the status and most recent findings with respect to audition have been written by Bárány (**6**), Newman (**111**) and Walzl (**159**). In 1950 the Psycho-Acoustic Laboratory of Harvard University published a bibliography on audition including over fifty-five hundred entries (**190**).

Methods

Discrimination mazes. For the rat, and in a few instances for other animals, the principal early method of determining tonal sensitivities involved some variant of a discrimination problem, in the form of a maze requiring choice between a tone-filled alley and a silent alley, or between two alleys with a relatively great difference in tone intensity (**68, 69, 70, 71, 72, 78, 79, 80, 81, 83a, 102, 148**). The more successful apparatus incorporated relatively intense electric shock. Henry (**69**) found that when a shock of 135 volts was given, discrimination habits were established in from 14 to 70 trials, while a shock of 40 volts had little effect. This may be contrasted with the experience of Thuma (**148**) on a slightly different maze, on which discrimination took between 710 and 1,410 trials, and with that of Muenzinger and Gentry (**102**), in which discrimination required between 80 and 250 trials. In the typical experiment the animal is required to go down the tone-filled alley to the food. A sound source is placed at the end of each alley; the apparatus

usually provides for silent switching-on of the sound to a predetermined intensity level. A serious objection to the two-alley maze arises from the fact that even the best sound-conditioning permits some sound in the "silent" alley. For example, Henry and Brown (**71**) found that while the active alley of their apparatus had an intensity level of 50 decibels at 2,500 cycles, the inactive alley had an intensity level of 38 decibels.

Conditioned response techniques. In the more recent studies with rats, and in the great majority of other studies, the conditioned response technique has been the preferred method. Britt (**15, 16**) and Cowles and Pennington (**24**) used "classical" conditioning, conditioning the rat's respiratory response and squeak, respectively, to tones. Conditioned shock-respiratory response has also been employed with cats (**173**), and guinea pigs (**77, 150**). Black and Schlosberg (**12**), Eccher (**38**), Gentry (**48**), Gould and Morgan (**50, 51**), and Hunter and Pennington (**82, 83**) established an "instrumental" conditioning with the rat, involving training it to move from one part of the apparatus to the other to escape shock. Dworkin and his associates employed a conditioned motor-alimentary response with cats and dogs: the animal was trained to obtain food in response to the stimulus tone by raising the hinged lid on the food box (**33, 34, 35, 36**). The method is described by Dworkin, Sutherland, and Seymour (**37**). Ades, Culler, Mettler, and their associates utilized a motor flexion response (**1, 2, 17, 26, 27**). The salivary reflex has been used in Pavlov's laboratory and by others (**4, 5, 120, 124**). Sutherland and Dworkin (**146**) compared results from the salivary and motor-flexion responses, and Dworkin (**35**) compared the salivary response with the alimentary-motor response. Elder (**39, 40**) conditioned chimpanzees to press a telegraph key at the sound of a tone to release food from a hopper. Harris (**66**) and Wendt (**172**) used conditioning techniques with monkeys to determine their tonal sensitivities.

The conditioned response technique, as applied to studies of tonal discrimination, has the following general characteristics: when the problem involves determination of upper frequency limits, conditioning is effected at a supraliminal frequency, and the frequency is increased until the response is no longer made. Occasional reinforcement (substitute reinforcement) at liminal values may be necessary to obviate the possibility of extinction. When the problem involves determination of an audiogram, training is effected at a supraliminal intensity at each frequency measured, and then the intensity is systematically decreased. To determine tonal differentiation, a positive response is conditioned to one tone and a negative response (inhibition of response) to another which is supraliminally removed in the auditory spectrum. Then the negative tone is gradually reduced (or increased) in frequency until the differential response fails. Since failure of differentiation is usually accompanied in each type of experiment by symptoms other than mere

extinction, such as tantrums, increased latency of response (**12, 66**), negativism (**5, 39, 40**), marked behavior disturbances (**36, 39, 40**), and the like, the determination of liminal values can be rather clearly effected.

Methodological problems. The conditioned reflex method of determining animal auditory spectra has been, in general, the most satisfactory and has given the most consistent results (**39**). It provides the closest approximation to the audiogram that can be obtained in experimental animals (**159**). Thresholds obtained from it have been lower than those from mazes (cf. **45**, and **12, 16, 24, 38, 51**). However, interpretation of these audiograms, especially in terms of comparison with human thresholds, is somewhat beclouded by two factors.

First, the available evidence shows that practice lowers the threshold and would therefore tend to provide estimates of the sensory capacity of the experimental animal that are at variance with its typical, i.e., unpracticed, capacity. Drury (**32**) found that trained telephone operators showed a 12.7-decibel advantage over untrained operators, and Elder (**39**) noted a 5- to 10-decibel improvement in two adult "controls" he employed in his study of chimpanzees. In animal conditioning experiments a considerable practice effect is possible. Further, the amount of practice (trials required for establishing a discriminating response) has varied with the animal, the technique, and particularly the frequency. Dworkin, Katzman, Hutchinson, and McCabe (**36**) reported that it required three to four months of training cats and dogs in the alimentary-motor response before testing could be done, while Lipman and Grassi (**98**) required only two to four weeks with dogs. Razran and Warden (**124**) reported that, in the Russian laboratories, some discriminations in the dog were effected in 10 to 100 trials, while others required between 100 and 1,000 trials. Hunter and Pennington (**82**) reported that it took 325 trials to train a rat to respond to a 1,000-cycle tone (and only 70 trials to train the animal to respond to a buzzer). Since, except in the audiometric laboratory, the animal has generally had no intensive practice in tonal discriminations, it would be desirable for many purposes to partial out the effect of practice on its audiogram and thus arrive at a "true" (i.e., typical) estimate of its auditory capacity.

Second, the observation of a number of workers that the auditory cerebral cortex is not essential for conditioning to a sound stimulus[1] (**123, 132, 159**) makes it important to know which auditory functions can be subserved by each level of the higher brain pathway if conditioning is to be used as a testing method. Further, Neet's (**109**) finding that, with human subjects, verbal report yielded lower thresholds than

[1] Sutherland and Dworkin (**146**), however, found that after bilateral surgical lesions in the auditory cortex of the dog, no clear-cut CR could be elicited by pure tones.

either a conditioned respiratory response or a manual motor response adds another caution to comparisons between animal audiograms based on conditioning experiments and human audiograms based on report.

Sound sources. Early experimenters used sound sources of variable acoustic properties, such as tuning forks, reeds, vibrators. With the development of electronically driven audio oscillators, tones of high purity may be readily generated (**25**, **71**). Furthermore, Dworkin's (**36**) objection to the effect that the characteristics of sound transmitters at high frequencies are not known is probably no longer valid. Particularly since World War II audio instrumentation and technology have made rapid progress (**111**).

Auditory Capacities of Selected Species

Audiograms have been determined under relatively comparable conditions, for infrahuman primates, dogs, cats, guinea pigs, and rats. The evidence indicates that the upper limit of the auditory spectrum of all these species (with the exception of the guinea pig and possibly the cat) is higher than man's, and that the intensity threshold is lower at the higher frequencies. Figure 1 presents typical audiograms for several species.[2]

Primates. The auditory capacities of monkeys were investigated by Harris (**66**) and Wendt (**172**). Intensity limens were calculated from 62.5 cycles to 8,000 cycles (**66**), and from 64 cycles to 16,384 cycles (**172**). Harris employed the psychophysical method of serial groups, while Wendt determined limens by setting the threshold at the lowest intensity to which the animal responded (omitting an ascending series). The results were highly consistent, however, particularly with respect to the shape of the audiogram. The monkey's thresholds were slightly higher than man's in the middle range (1,000–2,000 cycles) and considerably higher (9–25 decibels) at 4,000 cycles, where a marked "dip" was noted. The animals' threshold was lower (14–30 decibels) at 8,000 cycles and above.

Elder (**40**) placed the upper limit of the chimpanzee's auditory spectrum at between 26 kilocycles and 33 kilocycles (as compared with 22.6–23.7 kilocycles for three children), and determined an audiogram (**39**) essentially similar to that for monkeys, including the "4,000-cycle dip."

[2] These audiograms are approximate. In some cases, values were estimated from figures in the sources cited. Furthermore, the reference level presented here—average human sensitivity—is not constant for all studies cited. The audiogram for the rat is an average of those given by Black and Schlosberg (**12**), Cowles and Pennington (**24**), Eccher (**38**), and Gould and Morgan (**51**). Henry's (**70**) audiogram was too discrepant from these to warrant including in the average.

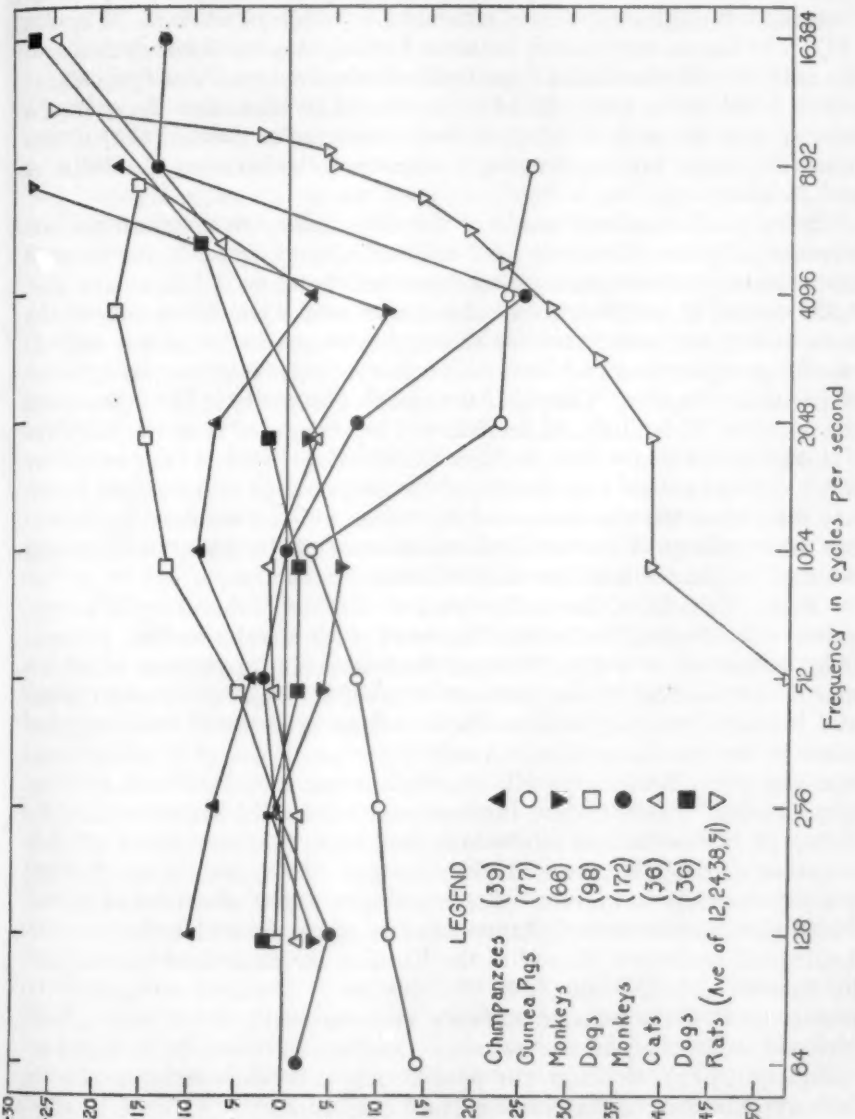

FIG. 1. COMPARATIVE AUDITORY ACUITY THRESHOLDS OF SEVERAL MAMMALS.

Cats. Using conditioning methods, the audiograms of cats were determined from about 100 cycles to 16,000 cycles (**1, 2, 33, 36, 109a**). Conditioned responses were elicited at frequencies as low as 30 cycles (**33**). The limens were lowest between 5,000 cycles and 8,000 cycles, and the animal's threshold was appreciably lower than man's at frequencies above 5,000 cycles (**36**). At 14 kilocycles to 16 kilocycles the animal's hearing was about 25 decibels better than man's. Wever (**173**) determined the upper limit of frequency response to be between 15 kilocycles and 20 kilocycles.

Some study has been made of the cat's ability to discriminate differences in tone. Dworkin (**34**) trained cats to make a conditioned motor-alimentary response discrimination between 2,150 cycles and 2,360 cycles, or a difference of about one tone. He also explored the cat's ability to make intensity discriminations and found for each of two frequencies that the limit of intensity discrimination was relative rather than absolute. That is, if the positive intensity is 54 decibels and the negative 50 decibels, 54 decibels will be responded to as the negative if it is preceded by the tone at 58 or 60 decibels. He noted that whenever pitch discrimination was developed in the presence of a notable intensity difference, the significance of the notes could be reversed by reversing the intensity. This finding is reminiscent of the relations obtaining between brightness and hue discrimination in the rat.

Dogs. Criticizing the earlier work of Pavlov's laboratory that suggested that the dog had a high degree of pitch discrimination, Johnson (**83a**) employed a Yerkes-Watson discrimination apparatus in an attempt to train dogs to discriminate between a "pure" tone and silence, and between two pure tones. He found no evidence of learning, and came to the conclusion that the animal's capacity for pitch discrimination was poor, if existent. His experiment was criticized both with respect to task difficulty and inadequately controlled sound sources by Anrep (**5**), who obtained a differentiation, using the conditioned salivary response method, between 637.5 cycles and 680 cycles. Dworkin (**35**) trained two dogs to make a differentiation of about one-third of a tone, which was consistent with Anrep's earlier study, and with the previous findings of Andreyev (**4**) and of the Russian investigations summarized by Razran and Warden (**124**). In this study Dworkin compared the results from a conditioned salivary response with those from a conditioned alimentary-motor response, and found them to be substantially equivalent, although the specific responses differed somewhat in latency, duration, initial activity, and inhibition.

The upper limit of the dog's frequency spectrum was placed at 36 kilocycles to 38 kilocycles by Andreyev (**4**), using a conditioned salivary response, and somewhat higher by workers in Pavlov's laboratory (**120**, p. 127). Sutherland and Dworkin (**146**) reported that their animals responded to sound from 50 cycles to 20 kilocycles, and in one case to

30 kilocycles. They demonstrated that dogs can differentiate between air-borne sound stimuli in the range 50 cycles to 300 cycles and tactual vibrations (when the table on which the dog is placed is set into vibration) at the same frequency.

Audiograms have been determined by Lipman and Grassi (**98**) and Dworkin, Katzman, Hutchinson, and McCabe (**36**). While the results are not completely consistent at all frequencies (the dog's thresholds reported by Lipman and Grassi are appreciably lower, from 200 cycles to 4,000 cycles, than those reported by Dworkin and his associates), the studies agree that human and dog thresholds were substantially the same below 250 cycles, and that the dog's maximum sensitivity was in the range of 4,000 cycles and above. Dworkin reported a superiority of 25 decibels at 14 to 16 kilocycles. Brogden and Culler (**17**) noted that the dog's auditory acuity was increased following roentgen-radiation of the hypophysis; the effect was due to induced hypoglycemia. The increase in acuity was most striking at the higher frequencies (8,000 cycles).

Guinea pigs. Herrington and Gundlach (**72**) reported definite evidence of discrimination in the guinea pig between tones of 500 and 600 cycles, and evidence of possible discrimination between 591 and 614 cycles. Their results were not conclusive, however, since they were not careful to use pure tones. Upton (**150**) obtained a discrimination between 600 cycles and 1,000 cycles, using a conditioned respiratory response. The discrimination required between 500 and 600 trials to establish. Horton (**77**) determined the animal's (seven animals) audiogram from 64 cycles to 8,192 cycles, using the thresholds of twenty human beings "tested under the same conditions" as a standard. He also employed a conditioned respiratory response. He found that the animal's threshold was higher than the human threshold except for a few cases at middle frequencies. Horton concluded that "the capacity of these animals in the perception of sound is similar to that of man." Analysis of his results in terms of the average of the seven-animal thresholds at each frequency suggests, however, that the guinea pig's discrimination capacity may be substantially poorer than man's at all but the middle frequencies.

Rats. The early negative results of Yerkes (**180**) and Hunter (**78, 79, 80, 81**) relative to the inability of the rat to differentiate pure tone are summarized in detail in Munn (**105**, pp. 140–145) as well as the first positive results of Muenzinger and Gentry (**102**) and Thuma (**148**). Both of these workers, employing electrically driven oscillators and simple mazes, demonstrated positive discrimination between a pure tone and silence. The history of the issue is brought up to date in Munn's recent volume (**107**, pp. 158–165). Since Wada (**152**), after careful study of the rat's inner ear, concluded that the animal possessed no peculiarity of structure that should result in lack of tonal sensitivity,

the later results seem to be more consistent with the facts. The difficulty Hunter encountered may be attributed to the difficulty of the task and inadequacies in the sound source used. Furthermore, the recent evidence suggests that the rat can successfully discriminate only relatively intense tones over 1,000 cycles; the apparatus used by Hunter was limited with respect to intensity of sound generated, or frequency, or both. The same difficulties probably account for Roth's (**133**) negative results with the hamster.

Several workers have determined the audiogram of the rat. Henry (**70**) used a discrimination apparatus to obtain thresholds for four frequencies between 1,000 cycles and 8,000 cycles. He noted that 1,000 cycles approached the lower limit of the rat's response to a pure tone, and that the animal's threshold is lower than the human threshold only at 8,000 cycles. At 1,000 cycles it required considerable forcing to get the animal to make a choice. Using conditioning methods, audiograms extending to higher frequencies (about 10,000 cycles) were determined by Blackwell and Schlosberg (**12**), Cowles and Pennington (**24**), Eccher (**38**), and Gould and Morgan (**51**). These findings are generally quite consistent with one another, and the trend is consistent with that noted by Henry. However, all the conditioning experiments yielded appreciably lower threshold values. The threshold is high at 1,000 cycles or less, and gradually declines until it is lower than the human threshold at 10,000 cycles.

It is likely that the upper limit of the rat's frequency response has not yet been recorded. Davis (**27a**), in 1927, reported that the harvest mouse responded to a 35,000-cycle tone generated by a Galton whistle. Gould and Morgan (**50**) report that the rat is sensitive to 40,000 cycles, and probably beyond.

Britt (**15, 16**) obtained differential conditioned responses to frequencies of 480 and 2,048 cycles, and to frequencies of 960 and 2,048 cycles, while Blackwell and Schlosberg (**12**) conditioned one rat to discriminate between 9,000 and 10,000 cycles, and another to discriminate between 8,500 cycles and 10,000 cycles. The evidence, therefore, indicates that rats differentiate between tones and silence over a wide frequency range, extending from about 1 kilocycle to far beyond the human frequency response limit, and that they differentiate tones varying in frequency.

Action Potentials and Auditory Capacity

The cochlear response has been measured for a few animals as a possible index to hearing. For the guinea pig and cat, Wever and Bray (**175, 176**) and Wever and Lawrence (**177**) found that the cochlear response bears a precise linear relationship to stimulus intensity over a wide range. The curves vary in form, in departure from linearity, and

in the maximum cochlear response attained. Below 1,000 cycles, larger cochlear responses are attained with increase in stimulus intensity, than with frequencies above 1,000 cycles, where a plateau in cochlear response begins at lower stimulus intensity values (**177**). Wever and Bray (**175, 176**) and Cowell and Black (**23**) deny, however, that cochlear response levels are to be identified with loudness levels. Galambos (**46**) obtained cochlear potentials from the rat up to 98 kilocycles (the limit of the equipment) and "not above 40" kilocycles from the guinea pig. The precise relationship of cochlear response levels to acuity threshold levels is not yet clear.

Neff (**109a, 110**) conditioned cats to pure tones, obtained audiograms, and then partially sectioned the auditory nerve. Audiograms on the operated animals revealed that while very small lesions resulted in no loss, larger lesions resulted in high tone loss or in loss at all frequencies. On the other hand, Wever and Neff (**178**) found, after partial section of the eighth nerve, marked high tone loss in only one operated animal, as measured by the cochlear potential.

The foregoing summary of the auditory capacities of infrahuman mammals suggests that a certain body of significant data has been accumulated. The range of capacity in most species is still to be explored, particularly with larger populations that will permit the determination of reliable averages. Problems arising out of the conditioned response method of determining audiograms must be answered. No reliable data seemed to be available for such common animals as the horse, sheep, cow, pig. Questions such as the effect of domestication on animals have not been resolved, and only a beginning has been made in the correlation of sensory capacity with such contingent behavior as perception and learning. It is clearly possible, however, to design experiments which will take into account the essential auditory peculiarities of the species for which data are given above.

IV. Gustation

Parker, in his model monograph in 1922, pointed out that the comparative physiology of taste in vertebrates was an almost untouched field (**114**, p. 166). Although many experiments since then have dealt with various aspects of gustatory sensitivity, as yet few generalizations of significance have been obtained from them (**101**, p. 156). Scarcely a score of papers exist that present relatively reliable evidence about the nature and limits of infrahuman mammalian taste sensitivities. These few provide fragmentary data concerning taste thresholds for a limited number of substances for perhaps two hundred animals primarily from

five species: the chimpanzee (117), monkey (13, 14, 117, 119), dog (138), rabbit (43), and rat (7, 117, 122, 126, 127, 128, 129, 130, 133, 165, 170). Furthermore, differences in treatment of data and manner of presenting results make interspecies and interstudy comparisons difficult. Brief reviews touching in part on infrahuman gustatory capacities have been written by Geldard (47), Patton (116), and Patton and Ruch (118). The fullest current account is perhaps that of Moncrieff (100).

Methods

The more precise among the experimental procedures employed to explore the limits of auditory and visual capacities have not yet been adapted to the determination of gustatory *sensitivity thresholds*, or the minimum stimulus values to which the animal can respond or which it is able to differentiate. Patton (117) has pointed out that no simple way of making reward and punishment depend upon correct discrimination of two taste thresholds has yet been invented.

The typical technique employed has involved determination of the concentration of test solution most frequently preferred by the experimental animals. This procedure, the *preference method* (7, 13, 14, 43, 117, 119, 122, 126, 127, 128, 129, 130, 170, 187, 188), has yielded *acceptance thresholds* (the maximum concentration of a noxious substance accepted as frequently as a neutral one, usually water, or the minimum concentration of an acceptable substance reliably preferred to a neutral one, such as water) and *rejection thresholds* (the minimum concentration of a substance consistently rejected in favor of a neutral substance such as water).

The preference threshold experiments have almost universally involved substances in solution, for they permit ease of measurement and control of texture.

In the typical experiment, a rack of funnels, water dishes, or other drinking vessels is placed in the cage. Two funnels or dishes have been used with rats, mice, rabbits; apparatus for the monkey has included as many as six, and for the chimpanzee, eight (117). The animal may be allowed to become accustomed to the experimental situation over a period ranging up to two weeks (125, 126), while records of normal water intake and solution evaporation are maintained. Most of the studies have used distilled water for the control solution, but Weddell (170) used tap water on the hypothesis that distilled water would "create a preference for quinine" (170). It may be noted that his threshold for the rat is almost twice the rejection threshold determined by Patton and Ruch (119).

After habituation to the apparatus, the experimental regimen is begun. The animal, which is kept on a standard maintenance schedule, is usually starved from early one evening until the following morning, when two solutions, water and a controlled concentration of the test solution, are placed in the cage. The concentration of the test solution is increased (or decreased) in small steps daily, until a threshold criterion is reached and passed. Daily records are maintained of water and test solution intake. If an ascending series is used initially, a descending series may follow; or the reverse procedure may be employed.

In an earlier and less well-controlled variation of the preference method, Watson (**165**) presented the animal with small cubes of bread soaked in a test solution. This method yielded much higher threshold values than the solution technique. Roth (**133**) offered hungry mice a choice of pairs of piles of grain dipped into various solutions. In the comparisons, the members of the pair were usually dipped into different solutions (e.g., saccharine solution and acetic acid). His method therefore gave values relative to the substances, both of which might have been equally noxious.

Young (**186**) has pointed out that preference experiments tell us what concentrations are consistently selected, but that this selection may depend upon a position habit, a food habit, or the palatability of the food. These environmental or organismic factors may be only indirectly related to the animals' gustatory discrimination capacity. Bare (**7**) noted that position habits affected the choices made by the experimental animals, and consequently the threshold determinations. Further, more fluid was ingested during the last six hours of darkness than during other periods. Beebe-Center (**9**) found that greater amounts of saccharine solution than water are ingested in the preference situation.

That preference experiments do not yield minimum sensation thresholds has been clearly indicated by studies by Bare (**7**) and Richter (**125, 126**) of the effect of adrenalectomy in the rat on the salt preference threshold. Whereas the preoperative thresholds were about 0.06 per cent, the postoperative thresholds were between 0.016 per cent (**7**) and 0.003 per cent (**126**). Richter (**126**) concluded that changes in the oral cavity resulting from the effects of adrenalectomy led to enhanced discrimination. However, Bare (**7**) found that the receptors were *not* affected: the concentrations of sodium chloride required to evoke action potentials in the chorda tympani were not significantly different in normal and adrenalectomized animals. Further, Pfaffman and Bare (**122**) found that systematic surgery on the lingual and glossopharyn-

geal nerves, leading to complete degeneration of the lingual taste buds (**116**), resulted in slightly elevated thresholds, but in an unchanged maximal concentration preference. They concluded that some sensory mechanism other than lingual taste is essential in initiating preference behavior. Finally, Young (**187**) obtained preference thresholds in normal rats of between 0.05 per cent and 0.01 per cent, depending on conditions, and further found that adrenalectomized rats do not ingest a significantly greater total amount of salt than do normal rats (**188**).

The matter has been summed up by Patton (**116, 118**), who pointed out that preference thresholds differ from absolute taste thresholds. He suggested that the fact that normal rats (as compared with adrenalectomized animals) do not prefer weak salt solutions to water may only mean that motivation to discriminate is insufficient. The salt deprivation brought about by adrenalectomy may then provide the motivation for finer discrimination.

Richter and Campbell (**128**) noted that the preference threshold of rats for sugar (0.5 per cent) was near the human *sugar recognition threshold* (0.41 per cent), but higher than the human *difference threshold* (0.17 per cent). Richter also found that the human salt recognition threshold was 0.087 per cent (cf. rat preference threshold of 0.055 per cent), while the human difference threshold was 0.016 per cent. In view of all the foregoing, however, it would be hazardous to conclude that animal preference thresholds are in fact functionally equivalent to human taste thresholds. The evidence suggests that, given adequate motivation (as in a reward and punishment situation), rat difference thresholds may be sensibly lower than any yet observed.

More direct methods of determining gustatory thresholds have also been used, but a translation into the terms of the preference experiments is hazardous without clearer evidence than is presently available of the correlation between neural and physiological functions on the one hand and taste sensitivities on the other. Both Pfaffman (**121**) and Zotterman (**189**) explored the gustatory sensitivity of the cat by recording action potentials from the chorda tympani and glossopharyngeal nerve following application of stimulus solutions to the tongue, and concluded that cats do not seem to discriminate sugar solutions. Pfaffman and Bare (**122**) found, however, that while salt *preference thresholds* were elevated somewhat after severing the glossopharyngeal and lingual nerves of the rat, the maximal preference concentration in operated animals was the same as it was in normals, even though there was complete degeneration of the lingual taste buds (**116**). Their conclusion that preferential taste behavior is initiated by other than the lingual

TABLE III

Mammalian Gustatory Thresholds*

(In grams per 100 cc., or in per cent solution)

Animal	*N*	*Substance*	*Acceptance Threshold*	*Rejection Threshold*
Chimpanzee (117)	1	Quinine	0.0003	0.0062
Monkey (13)	3	Quinine	0.0031	0.025
Monkey (119)	2	Quinine	0.0004	0.0015
Monkey (117)	15	Quinine	0.0031	0.025
Rat (117)	9	Quinine	0.00005	0.0003
Rat (170)	14	Quinine		0.0007%
Rabbit (43)	10	Sucrose	1.061	
Rat (128)	13	Sucrose	0.524%	
Rat (129)	7–10	Sucrose	0.57%	
Rat (187)	10	Sucrose	0.5%	80%
Rat (127)	17	Alcohol	1.8–4.8%	6%
Rat (129)	7–10	Maltose	0.06%	
Rat (129)	7–10	Galactose	1.6%	
Rat (129)	7–10	Glucose	0.2%	
Rat (129)	7–10	Lactose		2.2%
Rat (126)	20	NaCl	0.055%	
Rat (7)	10	NaCl	0.06%	
Rat (187)	10	NaCl	0.01–0.05%	
Rabbit (43)	10	NaCl		8.18
Rabbit (43)	10	LiCl		7.63
Rabbit (43)	10	$CaCl_2$		7.70
Rabbit (43)	10	KCl		8.20
Rabbit (43)	10	NH_4Cl		5.35
Rabbit (43)	10	Sodium acetate		10.67
Rabbit (43)	10	Sodium sulfate		9.23
Rabbit (43)	10	Sodium bromide		9.26
Rabbit (43)	10	Sodium nitrate		6.89
Rabbit (43)	10	Sodium iodide		11.84
Dog (138)	2	Lactic acid		0.83%
Dog (138)	2	Hydrochloric acid		0.31%
Rabbit (43)	10	Hydrochloric acid		0.437
Rabbit (43)	10	Acetic acid		1.02
Dog (138)	2	Acetic acid		0.156%
Rabbit (43)	10	Sulfuric acid		0.687
Rabbit (43)	10	Nitric acid		0.054
Rat (130)	47	Phenylthiocarbamide		0.0003%

* The original data in 13, 117, and 119 were in terms of gr/100 cc; in 43 they were in terms of normal solutions. Conversion of per cent solutions to gr/100 cc (or to normal solutions) is not readily achieved, since it would require knowing the volume occupied by the solute. Under, say, 0.5 per cent, the per cent figure will be only very slightly greater in absolute value than the gr/100 cc. At higher per cent values, the discrepancy will be relatively great depending on the density of the solute. The normal solutions were therefore converted to gr/100 cc, and the per cent values were included as given in the original reports.

apparatus has been cited above. Schellworth (**138**) made a study of the dog's sensitivity by noting, in animals with duodenal fistulae, the smallest concentrations of acids mixed with meat broth and bread that resulted in the pylorus reflex.

Treatment of Data

The preference technique has been characteristically employed with the psychophysical method of limits. However, calculation of the acceptance-rejection ratio and setting of criterion points has differed from experiment to experiment. Patton, Blum, Ruch, and Walker have determined the relative preference at each concentration of the test solution (t.s.) by the ratio

$$\frac{\text{total t.s.}}{\text{total t.s.} + \text{total water}} \times 100,$$

and they have apparently set the acceptance threshold at a ratio of 50 per cent, and the rejection threshold at or near zero per cent (**13, 117, 119**). Others, however, have used the ratio

$$\frac{\text{total t.s.}}{\text{total water}}$$

(**125, 127, 128, 129, 170**). With this technique, the threshold has been placed at the concentration at which the ratio is 1.0. Weddell (**170**) found that the ratio varied from 1.2 to 0.4 when both dishes contained water, and he therefore placed the threshold at the weakest concentration above a ratio of 1.9. With this criterion, he recorded, for 14 rats, an average threshold for quinine of 0.000718 per cent (while Patton and Ruch [**117**] set the threshold at 0.0003 gm/100cc, approximately half as strong a solution), and a range from 0.001125 per cent to 0.000375 per cent. In view of this spread of individual variability (which is not unusual—e.g., Patton and Clisby [**130**] found the preference threshold for phenylthiocarbamide for rats to vary from 0.02 per cent to 0.000005 per cent), and in view of the differences of calculation methods and criteria, current threshold data must be viewed with caution.

Gustatory Thresholds

Table III presents the thresholds for a variety of substances for the chimpanzee, monkey, rat, rabbit, and dog. The various investigators used different bases for expressing solution concentrations; the conversions effected here are only approximately correct.

It is evident from these data that meaningful generalizations con-

cerning the gustatory sensitivities of infrahuman mammals are virtually impossible. Too few animals and too few substances have been tested. Acceptance and rejection thresholds (as compared with sensory thresholds) have been demonstrated to be peculiarly susceptible to variation with intercurrent metabolic needs and maintenance schedules of different characteristics, and can but be ambiguous. Conclusions with respect to the relative sensitivities of man and animals, where human thresholds are almost invariably sensation thresholds (cf. **43**, **117**, **128**), are open to question. It seems quite likely that, in any given animal, the sensation threshold will be lower by some amount than either the rejection or acceptance threshold. Further, the range of intraspecies differences is such that the means from small samples are likely to be quite unstable.

V. Summary

The present article has presented a summary of the existing knowledge with respect to certain of the visual, auditory, and gustatory capacities of infrahuman mammals. While no pretense to completeness is made, it is believed that the larger part of the reliable evidence has been included. This survey has brought out three important points:

1. Only a few of the numerous infrahuman mammals have been studied at all, and of these only one or two have been studied with any degree of intensity.

2. The small sizes of samples employed, considered in the light of the usually large intraspecies variation, call into question the reliability of many of the findings.

3. Lack of comparability of methods used to explore the same capacity in the same or in different species makes it difficult to compare the results secured in these studies.

This paper has been concerned primarily with visual acuity, color vision, auditory frequency thresholds, and gustatory discrimination. It seems worth while briefly to compare the status of work in these fields.

1. It is the writer's judgment that the most reliable measurements are available for the largest range of animals with respect to audition. Consistent audiograms, based on essentially the same technique, have been obtained for the rat, dog, cat, and infrahuman primate. Both experimental method and instrumentation have developed to a point where some confidence can be placed in reported findings.

2. With respect to color vision, the findings reported are largely qualitative. In most instances it is safe to conclude only that the animal can discriminate some or all hues. The significant recent work has concerned only the rat and primates. It seems likely that the scattered early reports on other animals, such as the raccoon, porcupine, cat, guinea pig, squirrel, should be largely discounted.

Both the experimental methods and color sources used were not adequate by current standards. The history of color vision research with the rat suggests that negative findings particularly should be viewed with extreme caution. While color vision research accounts for a larger number of papers than any other area, only a small fraction of these are in any sense definitive.

3. As for visual acuity, not more than a score of individual animals seem to have been studied, and visual acuity in these was studied with a half-dozen different techniques, under as many different conditions. Both method and equipment are available, however, to undertake intensive comparative studies.

4. The area of gustation is difficult to evaluate. The preference technique, which has been employed almost exclusively, yields a measurement that cannot be legitimately construed as an index of minimum capacity to discriminate. Infrahuman mammalian audiograms, for example, are much more comparable to human audiograms than infrahuman preference thresholds are to human taste thresholds, even if it may be necessary to discount the effect of intensive practice with respect to the obtained audiograms. There are more or less *consistent* measurements available for one animal, the rat, for at least sodium chloride and sucrose. There are a few possibly significant measurements for the rabbit, and for primate sensitivity to quinine. More than in any other area, there is a need for a technique for gustatory research that will make possible determination of the *limits* of the animal's capacity to discriminate.

5. Finally, in no area considered has the problem of the relationship between the receptor mechanism and response capacity been more than broached. The locus of auditory threshold discrimination and of taste discrimination are both in doubt. The conflicting evidence with respect to the anatomy of the eye and color vision still remains to be resolved.

On the positive side, however, it may be noted that both stimulus-generating equipment and research techniques have been greatly improved and refined in the last ten years. The facilities are available for a full-scale attack on the problems adumbrated above.

The writer believes that such an attack is necessary. The essential reason for the study of comparative psychology is methodological: to study as completely as possible all the factors which underlie behavior, and to test and determine under controlled experimental conditions all the variables intervening between the presentation of stimuli and the measurement of responses. For many problems, the required degree of control can be achieved *only* with animals. However, before studying the "higher-order" problems of learning, perception, motivation, it is necessary to know, or make assumptions about, the contingent sensory response capacities of the subjects used.

In an experiment on human motivation, if we knew our subjects were blind, it is not likely that we would depend upon visual means of communication. It is not improbable, however, that we have made such mistakes in working with animals. It was the intent of this paper to point out the extent of our ignorance of the basic sensory capacity characteristics of animals, and to add a mite of motivation to attack the problems outlined.

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EXPERIMENTAL NAÏVETÉ AND EXPERIENTIAL NAÏVETÉ[1]

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It may seem to be a truism to assert that early experience in the life of any complex organism leaves an experiential residue which affects later behavior. Students of personality have amassed a considerable amount of evidence relating to the effects of early experience upon adult behavior in homo sapiens (11). In view of this impressive array of evidence indicating the importance of early experience upon subsequent behavior, few animal experimenters would deny that pre-experimental experience is important in the study of human behavior. With few exceptions, however, they have tended to overlook the possible effects of early experience upon the behavior of animals. They do not specifically deny that such a problem exists; they seem rather to be either unaware of it or to neglect it. It might well be argued that they hold an implicit double standard in their disregard of such factors in the study of animal behavior.

Beach (1) points out that 50 per cent of the reports on animal experiments in the *Journal of Comparative Psychology* and its successor, the *Journal of Comparative and Physiological Psychology*, since 1930 have been made on the Norway rat, although the latter accounts for only .001 per cent of all species available for study. Since the behavior of the rat has been most intensively studied and forms the basis for the most extensive contemporary learning theories, it seems well to focus attention on the area of rat experimentation. A casual perusal of the literature will soon demonstrate that the description of the subjects follows an invariant pattern: "the subjects were *N* experimentally naïve rats of such and such a strain." What, then, is the essence of experimental naïveté? Explicit is the fact that the animals have never been used in an experiment. Implicit in such a description are alternate premises: either that the animals' previous experience is of no importance whatsoever, or, that it is so uniform that every *experimentally naïve* rat is a standardized little mechanism subject to exactly the same pre-experimental influences in laboratories at California, Iowa, or elsewhere.

Results of recent research suggest that it might be well to scrutinize more closely the implicit assumptions made by experimentalists who

[1] The author is indebted to Charlotte E. Christie for surveying the recent literature and abstracting information pertinent to the pre-experimental care of animals.

utilize rats as subjects. As yet there is not enough evidence to specify precisely what sorts of pre-experimental experiences might affect the experimental performance of rats. There is enough evidence, however, to make it worth while to examine the provocative data. The present paper is therefore addressed to the practical and theoretical suggestions posed by these findings. It will be centered around two queries: (1) Is it legitimate to assume that all *experimentally naïve* rats will display similar behavior as adults? and (2) Does the use of *experimentally naïve* rats unduly limit the findings of experimenters?

The Comparability of Experimentally Naïve Rats

The bulk of studies published on animal experimentation in the journals of the American Psychological Association is carried in the *Journal of Experimental Psychology* and the *Journal of Comparative and Physiological Psychology*. In 1949 the former contained 18 articles describing experiments utilizing rats as subjects, the latter 38, making a total of 56 articles in this area. While scrutinizing these articles to determine the pre-experimental history of the animals used as subjects, certain interesting facts come to light.

Source. In 26 (47.2 per cent) of the 56 experiments scrutinized, *no mention was made of the source* from which the subjects were obtained. In some instances the assumption might be made that the animals were from the colonies maintained in the universities in which the studies were carried out. That this assumption need not necessarily be true is evident from the fact that in some studies in which the source was mentioned, the rats used were obtained from both the university colony and from animal supply houses as well as from other sources.

Nineteen (33.9 per cent) of the articles report that the animals were obtained from the colonies maintained by departments of psychology. In five (8.9 per cent) cases the animals were obtained from animal supply houses. Two (3.6 per cent) experiments report the use of rats obtained from nonpsychological colonies, viz., the Department of Agricultural Chemistry and the Home Economics Department. Two (3.6 per cent) other experiments utilized subjects who were partially from university colonies and in part from other sources—these in one case being procured from a local dealer, and in the other, they were wild rats from the Rodent Control Office of the City of Baltimore and the United States Public Health Service. Two (3.6 per cent) other experiments report the use of unselected stock from local dealers.

Aside from the wide variability of strains (which may or may not be reported) from such diverse sources, the factors of differential mainte-

nance schedules, caging, and handling must be considered. When rats from nonlocal colonies are used, there is also the effect of transportation to be weighed. At the University of California the author used in experiments 130 rats from the colony maintained by the Department of Psychology. These animals were obtained when they were four weeks of age and kept until they were about four months old. During this period only one rat died. More recently the author obtained 60 animals three and a half weeks of age from an animal supply house. Three of these animals died in transit, and two more died immediately afterward. Deprivation severe enough to kill 1 of 12 animals during transportation contrasts with the much lower rate found in animals from a local colony. It seems quite plausible that such a difference in amount of deprivation or hardship might have persisting effects upon the survivors.

Maintenance schedule. Information relating to pre-experimental maintenance schedules was extremely sketchy, being mentioned in only one series of experiments. In the absence of fuller information it is necessary to rely upon personal observation. In most laboratories a constant supply of water is available during the pre-experimental life of the rat. Feeding tends to be more varied. In some laboratories the animals always have a constant supply of food available; in others they are fed once a day. The type of food varies considerably among laboratories. In some cases rats are reared upon dry mash, some on wet mash, some on kibbled biscuit, while others have a basic diet to which greens are added. It is impossible in almost all cases to determine from the data given in articles anything about the pre-experimental deprivation schedule of the animals involved, the only safe assumption being that there is considerable variability.

There is one further factor in regard to maintenance schedules which may be of importance. The responsibility for taking care of laboratory animals varies quite widely. In some instances faculty members are responsible for the care and maintenance of the animals, sometimes graduate students have the responsibility, and in other instances nonacademic employees are in charge. It is somewhat dubious to suppose that all of the aforementioned would be equally concerned to see that the animals never suffered deprivation. Within the relatively limited knowledge of the author, there are several instances in which the individual in charge of the animals neglected to feed them on schedule, with resultant deprivation. There is also the factor of accidental blockage of water bottles, resulting in unscheduled thirst for the subjects. Since there is never a mention of such factors in experimental reports, it is impossible to determine the frequency with which they occur.

Caging. Again we suffer from an almost complete lack of information. Laboratories visited by the author vary in having from one to twelve rats per cage. There is no standard cage design so that the size of the cage as well as the number of animals per cage is an uncontrolled variable. The only safe assumption is, unless the description specifically says otherwise, that the animals were reared in cages of some sort.

Handling. Once again there is no comparable information upon which comparisons of pre-experimental life might be based. In many cases when rats are obtained immediately prior to the experiment, the experimenters themselves do not know the previous history of the handling of the subjects. Aside from deliberate handling of the animals to insure greater docility, which is sometimes reported, the nature of the cages used is, in many cases, a determinant of the amount of handling received by the animals. Some cages are constructed so that droppings fall through a mesh floor and it is consequently possible to rear animals without touching them. In other cases bottomless cages rest in pans filled with shavings so that it is necessary to transfer rats to other cages while cleaning the home cages. In this fashion the animals receive a good deal of unpremeditated handling.

Aside from the quantitative amount of handling, there are not inconsiderable differences in qualitative aspects. Some experimenters dislike rats and handle them most gingerly in a nervous fashion. Others are quite at ease with animals and allow them to crawl over their shoulders, nibble their ears, and are otherwise quite permissive in their handling. In uncontrolled observations the author as well as other experimenters have often been able to tell which experimenter has handled which rats by the differences in the quality of the rats' behavior when picked up, as well as the amount of freezing, defecation, and other indices of nervousness when the animals were placed in a maze.

The Equating of Experimental Naïveté with Experiential Naïveté

If anyone wished to maintain that all rats have similar pre-experimental experiences, the indicated paucity of relevant information certainly leaves the burden of proof upon the protagonist of this viewpoint. To judge from the superficial amount of information available, the range of variability in pre-experimental care is considerable.

The crucial question in this regard is whether the variation in the pre-experimental life of the normal laboratory rat has any effect upon subsequent behavior in experimental situations. If not, the preceding observations are only of cursory interest; if so, they have an important

bearing on the interpretations of experimental findings. Once again we are faced with a paucity of relevant information. There is, however, enough evidence regarding the effects of pre-experimental variables to suggest that the problem is a valid one.

The failure to specify the source of the subjects used in rat experiments may in certain instances conceal the fact that animals of different strains are being used. Pigmented rats have better vision than albinos and would be expected to do better in experiments in which visual cues are important. Some strains have been bred for specific sorts of abilities and will perform differently in various situations. Tryon's maze-bright and maze-dull rats were bred for performance in a tunnel maze. Krechevsky (**8**) found that they were responding to different types of cues. He argued that the maze-bright rats were relying upon spatial rather than visual hypotheses in problem solution.

Another unknown factor would be differential breeding in different laboratories of animals originally obtained from a common strain. How comparable are contemporary rats whose remote ancestors were of the Wistar strain? Obtaining animals from different sources also means that the previously cited variables of caging, maintenance schedules, and handling are often unknown to the experimenters themselves.

Hunt (**5**) gave rats experience in food deprivation when they were quite young. These rats were found to hoard significantly more pellets as adults than were nondeprived control rats and animals which had experienced an equivalent amount of deprivation later in life. This suggests that experiments involving hunger as motivation might be affected by early deprivation.

An experiment by Christie (**2**) indicates that early deprivation is important in an experimental test of latent learning under reversed motivation. In a modification of Kendler's (**7**) variation on the Spence and Lippitt (**14**) experiment, it was found that pre-experimental deprivation training had marked effects upon adult performance. The basic design of the experiment involved a single choice point maze with food in the right-hand goal box and water in the left goal box. Animals were trained under hunger motivation while satiated for water. They received an equal number of experiences in both sides of the maze by the application of free and forced choices. In two experiments[2] by the author with subjects which had experienced a "normal" rearing in cages with food and water constantly present, it was found that their behavior con-

[2] An investigation of the role of drive discrimination in learning under irrelevant motivation, *J. exp. Psychol.*, 1951, **41** (in press); A further examination of the nonreinforcement position in learning under irrelevant motivation (in preparation).

formed to that reported by Kendler. On test trials the animals would continue to the previously rewarding right-hand side whether motivated by hunger or thirst. Two groups of rats which had been subjected to systematic pre-experimental deprivation of food and water extending between the thirtieth and ninetieth days of life displayed significantly different behavior when trained and tested under the same conditions. These animals tended to prefer the left-hand goal box containing the water bottle even though they had never been rewarded at that point.

These experiments indicate that extensive deprivation in pre-experimental life will have effects upon adult behavior in certain sorts of experiments. The amount of deprivation necessary to produce an effect and the sort of experimental situation in which such early deprivation might be of importance are as yet unknown. The possible effects of accidental deprivation or different forms of maintenance schedules cannot be specified, although they may be important uncontrolled variables.

As far as the author knows, there is no direct evidence bearing upon the possible effects of different sorts of caging upon the behavior of rats, with the exception of one experiment by Patrick and Laughlin (**12**), who reared rats in two different sorts of cages. A control group was reared in mesh-walled cages of the sort which are generally used. An experimental group was reared in special cages with glass walls and without any shadow-producing matter in the cages. When tested for wall-seeking behavior as adults, the experimental rats showed significantly more exploratory behavior in open places. Whether rats reared in large cages would learn different things from rats reared in small cages is a moot question. Ligon (**9**) has shown that a cage mate is effective as an incentive in maze learning. Maltzman (**10**) mentions a number of as yet unpublished studies conducted in Spence's laboratory utilizing a social cage, i.e., one with cage mates, as an incentive. Possible socialization effects resulting from different numbers of rats being reared together would seem to be worthy of investigation.

The evidence on the effect of handling is, fortunately, experimentally documented. Karn and Porter (6) compared a number of pretraining experiences: (1) habituation to handling, (2) familiarity with the maze, (3) familiarity with the maze and goal orientation, and (4) habituation to being detained in an enclosure similar to the starting compartment in the maze. It was found that all of these conditions led to more rapid learning in subsequent experimentation. The first mentioned is most pertinent to the present discussion, since it has been already pointed out that differential amounts of handling might occur both deliberately and accidentally.

Although the amount of evidence is scanty, it does indicate that perhaps one of the reasons for the frequent inability of experimenters to obtain comparable results in similar experiments might be the unequal sorts of pre-experimental influences resulting from differences in source, maintenance schedules, caging, and handling. Animals from a wide variety of backgrounds are all *experimentally naïve;* this does not automatically make them *experientially naïve,* nor should it imply that their pre-experimental histories are equivalent.

To assist in the evaluation of experimental results, it is suggested that the following data relevant to pre-experimental variables be included in experimental reports of rat experiments:

1. The strain, source, and age at weaning of the animals.
2. The pre-experimental maintenance schedule.
3. The type of caging utilized.
4. The amount of handling received by the animals prior to the experiments.
5. Any accidental deprivation or other possible traumatic experience.

If these data are included in all reports, it might be possible to clarify the reasons underlying some of the differences in results reported in similar experiments. It has long been a source of wry amusement that Iowa and California rats often perform quite differently in comparable experiments. In this context differences in the pre-experimental treatment of rats are interesting. California rats are of the Tryon strain, are reared in cages from which the animals have to be removed approximately once a week while the cages are being cleaned, and are reared on dry mash. According to Professor Kenneth W. Spence (personal communication) Iowa rats are of Hall's nonemotional stock, are reared in cages which can be cleaned without handling of the rats, and are reared on pellets. In both laboratories the usual number of rats per cage is four, so one variable appears to be controlled. Whether these differences in pre-experimental life are related to differing performance as adults is a moot question. Karn and Porter (6) found that only one transfer from the living cage to another one and back once a day for six days prior to experimentation was sufficient to increase the "latent" learning ability of rats. The California rats are normally handled a dozen times or so prior to the commencement of experimental training, in contrast to two or three handling experiences of the Iowa rats. Both groups would, of course, normally receive handling the week prior to actual experimentation in order to increase docility. It is questionable whether this would suffice to equalize the effects of differential amounts of prior handling.

The author does not wish to espouse a rat phenomenology. It is sug-

gested, however, that experimenters consider the extremes of the range of restriction of stimuli present in the pre-experimental life of the rat and reflect upon the relatively great importance which variation in these might have upon the rat's life. The more standardized the pre-experimental influences, the more valid the comparison of results obtained from different laboratories. If differing results are still found, the search for the responsible variables can be legitimately delimited to those present in the experimental situation itself.

Possible Limitations Arising from the Study of Experientially Naïve Rats

If, as the preceding discussion suggests, unintended variations in the pre-experimental life of the laboratory rat may be enough to create important differences in experimental behavior, an important question may be raised. What would be the effect of deliberately introducing drastic differences in the pre-experimental world of the rat?

The pertinence of this query arises from the fact that the average laboratory rat is reared under unique circumstances. He is confined to a relatively small area in which he has to engage in simple operations in order to satisfy his needs. The range of potential stimuli to which he is exposed is arbitrarily limited. The activities which, under more varied environmental conditions, might be learned as means of relieving physiological tensions are not developed. His world is relatively static and is not conducive to the full development of the rat's potential behavioral repertoire.

Comparable restriction of experience on the human level is extremely rare. The findings of the retardation of the children in Colvin Hollow (**13**), where they experienced a good deal more freedom than the average laboratory rat, illustrates the effect of relatively meager environmental stimulation. Feral man has not proved adaptive to the man-made mazes of civilization. If the patients of Freud had spent their infancy and adolescence in cages, it is interesting to speculate as to the sorts of personality mechanisms he might have uncovered. As it is, he has been criticized for drawing upon maladjusted, bourgeois Viennese for principles which were generalized to all mankind. If this criticism has any legitimate basis, it is difficult to justify the study of cage-reared rats in an effort to understand the behavior of rats reared in a much richer environment.

The hard-bitten rat-runner might agree with the foregoing comments, but could point out that the simple pre-experimental life of the average laboratory rat eliminates many extraneous variables and leaves

the basic ones open for investigation. This is a perfectly legitimate position, although the preceding section indicates that it may be a dubious assumption in view of the uncontrolled variability that does exist. In the initial stages of theory-building simple beginnings are undoubtedly best, but it is the responsibility of the theorist to demonstrate that the principles uncovered in the study of rats with a restricted background are applicable to the behavior of rats reared under more varied environmental conditions. In the opinion of the author, this should be done before application of the principles is made to human behavior.

There is experimental evidence relative to the subsequent behavior of rats subjected to varying degrees of pre-experimental richness of environmental experience which indicates that it differs from that of rats reared under "normal," i.e., cage conditions. The experiments are few in number, but their implications cannot be neglected. In discussing them, reference will be made to quantitative and qualitative differences in learning ability. By *quantitative differences*, we mean that the behavior of the rats with richer experience differs from that of experimentally naïve rats, but only in degree. Thus, although faster or slower learning might be manifested, it would not invalidate the principles governing the behavior of the usual laboratory rat. "Qualitative differences in learning . . . " means that the behavior displayed is interpreted as involving additional principles necessary to explain the differences in performance between the two groups.

Hebb (**4**) reports two exploratory studies which are relevant. In the first he contrasted the behavior of two groups of rats, one subjected to "normal" cage conditions, the other to an even more restricted pre-experimental life. The latter group was blinded in infancy, but otherwise was reared under conditions identical to the first group. The animals reared under "normal" conditions were blinded at the age of three months. When both groups were five months of age they were tested in a series of experiments. It was found that rote learning tasks did not differentiate the two groups. In a task involving a change of response given at eight months of age, it was found, however, that every late-blinded animal did better than every early-blinded animal. Hebb interpreted this as indicating the permanent effect of early experience upon problem-solving ability at maturity.

The finding that animals subjected to meager early experiences did more poorly than animals subjected to "normal" rearing suggested a further exploratory study. What would happen if "normal" rats were contrasted with a group of rats who had richer early environmental experience? Hebb took seven animals home and reared them as pets.

During this period of time they experienced a good deal of time outside their cages, running about the house. When these pets were compared with cage-reared rats in a total of 21 test situations, it was found that the pets all scored in the top third of the total distribution for cage-reared and pets (25 cage-reared animals were used). Inasmuch as Hebb does not describe the testing situations, it is impossible to determine whether there were qualitative as well as quantitative differences in their problem-solving behavior.

An experiment by Christie (2) indicates that qualitative differences do appear, at least in certain experimental situations. In a modification of Kendler's (7) experiment previously mentioned, a group of rats which had been given extra-maze exploratory experience was tested. The exploration training was given under satiation on an elevated runway and an open table top. Food and water were present on the table, and the animals received 20 exploratory experiences of 24 hours' duration between the ages of one and three months. When these animals were tested under conditions identical to those previously alluded to, it was found that they displayed behavior significantly different from that of cage-reared animals which also had never experienced deprivation. The early explorers did go to the water bottle and drink when thirsty, as contrasted with cage-reared rats which continued going to the goal box containing food where they had been previously reinforced. Richer early experience in this case quite definitely led to qualitatively different behavior.

These findings led to another series of experiments (not yet prepared for publication) designed to determine what effects early exploration would have upon the behavior of rats in place-learning experiments. Rats which were given extensive experience upon changing configurations of tables connected with runways were tested, as were cage-reared controls. Food and water were always present on one of the tables and in the cages. The exploratory training extended through the second and third months of the rats' lives.

The first experiment required the animals to travel from a starting point fixed in space to a goal which was also fixed in space. The route between was a multiple T-maze with nine choice points. The maze was shifted on every trial so that the animals never traversed the same path from the starting point to the goal. Under these conditions it was found that both groups of animals displayed indications of place learning but that the early explorers learned more rapidly, so the differences were quantitative rather than qualitative.

In a second experiment the location of the goal was fixed, but the starting point was shifted in space. The route to the goal also varied from trial to trial in this experiment. After a training series the animals were placed upon a new starting point and had an option of choosing from a number of radiating paths, one of which led directly to the goal. The cage-reared animals made chance re-

sponses to the correct path, whereas the early explorers chose the correct path at a significant level. In this situation we find qualitatively different behavior differentiating the animals on the basis of their early experience.

In a third, still more complex situation, neither group gave significant evidence of having learned the problem. Thus, in situations of varying difficulty, it was found that in the simplest the early explorers displayed a quantitative superiority, in a more complex situation a qualitative superiority, and in a very difficult situation no difference in performance was demonstrable.

Interesting differences in the rats' behavior during all the testing were observed, these differences bearing no known relationship to learning criteria. At the inception of the experiments, all rats were given four practice trials on running down runways to food. The early explorers were undisturbed by this training. The cage-reared animals behaved as might be expected. They were quite excited, and there was a high incidence of freezing and defecation. They would not voluntarily advance down the elevated runway but had to be coaxed.

For all three experiments the median time per trial was plotted against the number of trials. Both groups show a typical performance curve in running-time for all three experiments—starting slowly and then reaching a plateau. This plateau was reached after an equivalent number of trials for both groups. Of special interest is the fact that the running-speed of the early explorers was consistently twice as fast as that of the cage-reared animals during all three experiments. At the completion of the last experiment the cage-reared animals had had a month's daily experience on elevated mazes; yet the difference in running time was still significantly faster for the early explorers. Quite extensive adult experience on elevated mazes on the part of the cage-reared rats was not sufficient to compensate for the early exploratory experiences received by the experimental group.

This difference in gross behavior of the two groups was so great that observers who had never seen rats run a maze could readily differentiate between the two groups on the basis of the manner in which they ran the maze. The early explorers were not only much faster but ran the maze in what might be described as a "slap-dash" manner. When approaching choice points (all of which were new to them on every trial), they displayed little hesitancy and would often make a choice without slowing down. An indication of this abandon is illustrated by their behavior when they chose blinds. They would occasionally be running so rapidly that they would be unable to stop at the end of the blind alley and would skid over the end of the maze and slide half-way down the support to the floor before they could stop. They would then climb back up to the maze and start running again, with no observable effects on their subsequent behavior. The cage-reared rats never displayed such behavior. No cage-reared rat ever ran off the end of a maze. Cage-reared rats also displayed different behavior at choice points, since they tended to be much more hesitant with a good deal of freezing and "VTEing."

If further experimental work confirms the initial finding—namely, qualitatively superior behavior in problem-solving situations when rats with a wide degree of pre-experimental experience are used as subjects—certain implications follow. A greater range of observed behavior leads

to a broader empirical basis for theoretical conceptualization. Just as the factor analyst is limited in his findings by a narrow range of behavioral correlations, so is an animal theorist limited by the narrow range of behavior exhibited by cage-reared rats.

A further area which could profitably be investigated would be that of various genetic hypotheses in which rats are used as experimental subjects. A good deal of the emphasis in personality theory has been on developmental aspects of learning. These have been, as Gibson (3) points out, relatively neglected in the area of animal experimentation, Hebb (4) being a refreshing exception. Many possible permutations of early deprivation and types of exploration and learning are possible which woud be of considerable potential value in clarifying various relationships of early experience to later behavior.

The continued equating of *experimental naïveté* with *experiential naïveté* means quite bluntly, in the author's opinion, that animal experimenters are not studying the principles of behavior, nor the principles of rat behavior, but the behavior of cage-reared rats. For many purposes this is quite adequate; for a more generalized theory of behavior it seems to fall short of asking what seems to be an extremely important question: What are the effects of earlier experience upon subsequent behavior?

Summary

A survey of recent articles on rat experiments reveals that very meager information is available on the pre-experimental life of the *experimentally naïve* subjects. As far as can be determined, a considerable amount of variability in treatment occurs in various laboratories. Experiments were cited which indicate that these variations might have an appreciable effect upon the behavior of adult rats and might underlie some of the contradictory findings reported in the literature. Suggestions for additional data on the pre-experimental care of rats were made so that comparisons between experiments might be more validly made.

It was further argued that the exclusive use of cage-reared rats might unduly limit the body of behavioral data, thus having a restrictive influence upon the theoretical inferences made from analyses of the data. Recent experiments were reviewed which indicate that rats with early exploratory training exhibit as adults qualitatively different behavior from that of rats without this early training. Further research designed to ascertain the effects of differential pre-experimental training is needed.

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THE USE OF EXTENDED CONTROL-GROUP DESIGNS IN HUMAN RELATIONS STUDIES

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Most psychologists who have conducted experiments in complex social situations wherein a single control group was used, have wondered privately and publicly as to the exact genesis of an observed change in their experimental groups. Several alternative explanations usually seem to be required to satisfy the wonderment. These are quite likely offered because of the psychologist's awareness that complex variables have been continuously operating and could not be cut down to laboratory size without distortion of an unknown magnitude.

While further experiments upon hypotheses derived from obtained changes are certainly desirable in order to specify the ways in which these variables are operating, the complexity and practicality of the situation, e.g., in industry work groups, is often such that subsequent experiments cannot be carried out.

What is needed are more designs and methods which will yield estimates of the operations of several variables within one experimental period, since subjects drawn from a complex societal group often reach a satiation point with one experiment.

A pertinent example of such problems is found in the rather poorly defined area of human relations. Experiments in this area primarily deal with some obfuscated relationships among persons interacting according to some psychosocial system, e.g., superiors-subordinates, members-group, members-nonmembers, husbands-wives, professionals-nonprofessionals, participants-reactivists, elected leaders–appointed leaders, etc.

Usually most psychological experiments in this area are set up to effect some kind of behavior change with a view to determining the producers of such a change. Unfortunately, these experiments often yield responses from the subjects which are difficult to relate to antecedents. For example, an experiment on factory absenteeism may have the hypothesis that a "high" rate of absenteeism is associated with intragroup conflict. All groups are compared on absenteeism rates, and those with similar "high" rates are studied further. Accordingly, systematic surveys and interviews are conducted to locate two groups—one experimental and one control—equally matched on mean intragroup conflict ratings (assigned scores to each individual on a given scale).

This constitutes the pretest. Then, a series of group meetings, led by a skilled, permissive, "democratic" leader, directed toward reduction of conflict, is used as the independent variable in one group, the other group remaining as a control. The posttest involves a repetition of the pretest, measuring the extent to which changes have taken place in conflict scores assigned by raters, and also observing the absenteeism rate. Let us suppose that little difference in change is found between experimental and control groups' results on either count and that the circumstances are such that it is not possible to modify or replicate the experiment on the same groups.

Granting that the matching variables also constitute the pretest, the fact remains that in this hypothetical experiment the usually effective techniques, for reducing conflict at least, failed to work. The experimenter may reason along several different pathways and not meet with an hypothesis that will meaningfully account for the results.

Recently, Solomon (8) has made a suggestion for the use of additional control groups in certain kinds of experimental studies. This extension is particularly applicable to studies in the area of human relations, especially where only a single experiment is possible.

He proposed an extension of the traditional single control-group design that allows for estimates of some of the variables arising in the experimental situation which are not easily noted in a single-control design. This involves the selection of a third group (called the second control group, C2) which does not receive the pretest (being assigned the mean of the E and C1 values), but which receives the same treatment as the experimental group and the posttest. A value *I* (interaction effect) is obtained by adding the amount of change taking place in the C1 and C2 groups, and subtracting this from the amount of change obtained in the E group. Solomon reports that as yet there is no means for reporting significance, only magnitude.

Schematically, his design is as follows:

	Experimental Group	*Control Group 1*	*Control Group 2*
Pretest	Yes	Yes	No
Training	Yes	No	Yes
Posttest	Yes	Yes	Yes

It should be noted that he also provides a four-group design, wherein a C3 group is given the posttest but neither the pretest nor the experimental treatment.

The essential reasoning behind his extension is that there are effects of the pretest combining with the independent variable and subsequently interacting with the posttest. These interaction effects seem to arise

from attentional and/or perceptual shifts due to the pretest. Using training as an example, he presented several assumptions: (1) that the improvement of C2 over its assinged mean is due to training alone; (2) that the improvement of C1 is due to the facilitating effects of the pretest alone; and (3) that the possible failure of the improvement of the experimental group to be equal to the sum of the improvement of C1 and C2 is a reflection of the interaction effects of the pretests. Now, the value *I* may be positive or negative in sign. If positive, then it would appear that the pretest somehow had a beneficial effect on the results of the training procedures. If negative, then the pretest may have developed some kind of resistance or interference: emotional, attitudinal, perceptual, or general orientational "sets."

The value of *I* may well be related in some fashion to Harlow's (6) learning sets, i.e., readiness, or learning to learn; to Bateson's (1) concept of "deutero-learning"; to Cronbach's "test response sets" (5); and to the somewhat neglected "principle of expectancy" as shown in such studies as those of Cowles and Nissen (4) and Tinklepaugh (9). In the latter's experiment monkeys rejected lettuce when they were apparently "expecting" a banana, though they normally accepted lettuce. In our hypothetical experiment perhaps the experimental group might reject participation in discussions if they were led to expect lectures in "how to get along with people," etc. At any rate, the value of *I* may possibly be a quantifiable expression of such factors as learning contexts, the operation of expectations, sets, and so on.

Applied to our hypothetical problem, this design would provide an inference as to whether the pretest in some fashion operated unfavorably upon the E group meetings. For example, the nature of the interview might have led the participants to anticipate that certain points would be brought out in the discussion in the same way they were in the interview. When these did not materialize in a recognizable form, the participants might easily have cast out the whole proceedings as a waste of time. Since these factors could not hold for the C2 group, this group might engage very adequately with the points brought out and find means for resolving the group difficulties. Further, absenteeism might well be reduced in this group (providing the hypothesis were tenable), the group members accepting nonconflict roles, being motivated to maintain a conflict-free group, and so on.

Within our hypothetical human relations experiment, one difficulty is pointed up in a two–control-group design, How would we select our C2 group? The method deliberately chosen was that of systematically surveying groups and finding two groups with equal absenteeism rates

to be further matched on the variable of intragroup conflict, this constituting the pretest. But C2 is not supposed to have the pretest. It would seem that there are several ways at hand for obtaining this group.

1. One involves the judgment of a person or persons sufficiently familiar with the various groups to be competent to designate the C2 group. But in human relations studies, this presents a very difficult assumption due to the nature of the variables. The superintendent of a factory may be able to show absenteeism records, but he cannot be expected to rate conflict on a common basis. His communication usually depends upon his subordinates, who are often motivated in ways at variance from the existing situations, i.e., the subordinates may not like others to know of the existence of intragroup conflict.

2. A second method for obtaining the C2 group would involve random or stratified sampling, drawing persons from various groups. These persons should not be from groups having had the pretest. However, this would mean individuals who did not know each other or who knew each other only slightly. The conflict variable could hardly seem relevant here. Only an assumption that they would be in conflict were they to be formed in a work group would make this selection tenable (though the writer often feels that this assumption would be entirely in order!).

3. A third way would be to select persons matched on any number of variables with the E and C1 members. For example, age, education, sex, length of service, ratings, occupational position, absenteeism record, etc. However, these variables may in no way be correlated with social interaction producing conflict. This could be determined, of course, by pre-experiment studies after the E and C1 groups have been chosen. But the likelihood of other than chance relationships seems small.

4. A fourth method, a variant of the way in which the E and C1 groups were obtained, would be to find the groups with equivalent absenteeism records and attempt to match the group's histories with those of the E and C1 groups. Many different sources would be utilized: the opinions of all persons familiar in any respect with each group, leadership histories, exit-interview results, production variability, medical records, variability in size, long-term reputation among other groups, and anything else that might be relevant. However, these would provide equivalent problems to those raised in the third method—pre-experimental studies would probably be required. This method does have, however, the advantage in that these variables seem to have greater face validity.

5. A fifth way would be to use free-floating trained observers who, without using the formal pretest, might uncover a likely-looking group. But conflict has an elusive quality when being observed. The shop gets cleaned up in a hurry when officialdom is suspected.

Thus, for human relations studies wherein the variables are quite complex, this design has an immediate difficulty inherent in it. Nevertheless, Solomon's suggestion provides a generalized design urgently needed for such studies and should engender a great deal of further research.

An Example of the Use of the Two-Control-Group Design

The writer elsewhere (**2, 3**) has reported in detail a study of the development and evaluation, by tests and other measures, of a human relations training course for executives and supervisors within the home office of three insurance companies. As part of the evaluation, a C2 group was used. While not of the order of the hypothetical experiment discussed above, certain aspects are relevant.

As in most industrial studies certain problems are inherent in the working situation. The relevance of any demands upon the working time of employees is properly of concern to executives, for example. In this study, it was thought that any course involving "psychology" and "human relations" should be fully understood by the executives prior to being tried out experimentally with their supervisors. Hence, a series of ten sessions involving about 45 executives was held. Then the experimental program was launched. All supervisors from one department formed the E group, and all supervisors from two other departments formed the C2 group, these latter departments, combined, equalling the E department in size. All three departments performed identical work, faced the same kinds of problems, and were very comparable in many other respects.

While the general design was satisfactory, there was one important problem. The E and C1 groups were both selected from departments whose executives had attended the executive training series. Ideally, none of the executives would have received training in the control departments. As noted above, it was considered important that the executives hold positive attitudes toward the training. To choose an E or C1 or C2 group whose executives were not familiar with what their subordinates were being requested to do was considered dangerous to the experimental study. Negative attitudes could well have had vitiating effects upon measured performance. Hence, improvement in the performance of the C1 group could have taken place through the communication of information and "democratic attitudes" from the trained executives to their untrained supervisors. This would then result in an underestimation of the improvement of the E group over the C1 group.

A C2 group was then selected. Faced with the same problem, that the executives had to be informed, the experimenter enrolled only supervisors whose superiors had attended the preliminary series. (The course was apparently quite popular—many more requests were received for enrollment than could be granted—especially from departments whose executives had attended the first series.) Again, ideally none of these

supervisor-executives would have been trained. Since the problems involved in the selection of this C2 group had not occurred to the writer at that time, he accepted one supervisor from each department, with the exception of three large departments (100 or more employees) wherein two each were accepted. Also, since all members of the C2 group were from different departments, the design was further weakened. This general selection method corresponds most closely with the second selection method discussed above.

The final N was 18 in each of the E, C1, and C2 groups. The groups were found to be fairly closely matched on means of age, education, mental alertness test scores, and years of service, and approximately equal in proportions of males and females.

A total of 12 separate scores was obtained from the measurement battery used. The value of I has been determined for 9 of the measures. To reduce discussion, Table I has been prepared showing the obtained I values and t values.[1] (A full description of the measures can be found in reference **2**.)

From these results it seems that differential effects of the pretest

TABLE I

MAGNITUDE OF OBTAINED I VALUES BETWEEN PRETEST AND POSTTEST SCORES AND OBTAINED t RATIOS BETWEEN PREDICTED AND OBTAINED POSTTEST SCORES

Measure	I*	t†
General Facts and Principles	−2.98	0.0001
How Supervise? (total)	−0.23	.0411
How Supervise?		
I. Supervisory Practices	+0.67	.2039
II. Company Policies	+0.72	.0465
III. Supervisory Opinions	−1.61	.0760
General Logical Reasoning	+0.05	.1805
Social Judgment Test	−2.03	.3050
Test of Ability to Estimate Group Opinion:		
I. Department Error	+7.75	.0270
II. Department Bias	−7.30	0.0241

* I = interaction effect of pretest and training on posttest scores.

† t = ratio of standard error of obtained differences between predicted and final posttest scores to its mean difference.

[1] Student's t was used, being obtained from a comparison of predicted and obtained means of the E group. Each predicted mean was derived from the regression of the C1 posttest scores on its pretest scores according to a covariance design presented by Peters and Van Voorhis (7, pp. 361–364). The C2 group did not enter into this analysis.

can be inferred in several measures. A brief discussion of a few of the individual tests will assist in gaining some insight into the nature of these effects.

The General Facts test is a short test of general psychological knowledge. Few of the trainees had any information about psychology. But they appeared to hold some fairly definite opinions about what it was. Hence, the structuring presumably given by the pretest could be assumed as establishing a "set" which, interacting with their preconceptions, might have served to make the course somewhat confusing to them. They gained significantly over their controls, but perhaps not as much as possible.

The "Supervisory Opinion" section of the File-Remmers How Supervise? test was quite similar in content to the General Facts test. The same result may have held here. The other two sections of this test seemed to reveal that the pretest established a facilitating orientation for the material obtained in the training, while the C2 subjects did not have this experience.

The Social Judgment test was acknowledged when taken the first time by the trainees as being quite difficult and somewhat ambiguous to them. Very few complained about it on the posttest, however. It did not reveal any significant improvement, as shown by the t value. Yet a sizeable I was obtained.

Two rather surprising findings occurred in the two subtests of the *Test of Ability to Estimate Group Opinion*. Simply stated, the error score represented how many percentage points the supervisors were in error in estimating the percentage of persons in their departments holding certain opinions (morale scores) about working conditions, company policies, community, their jobs, their supervisors, etc. The bias score represented the direction of their errors, i.e., underestimation or overestimation of the percentage. Apparently the E supervisors were more correctly oriented as a result of the pretest as far as error was concerned, but the over-all bias (which was a sizeable underestimation of morale) did not improve as much as possible. Again, this was seemingly a function of the pretest effects. These results lead to an hypothesis that experience in judging morale has the effect of making the judge at once more sensitive to the group's opinions, but also more "defensive" as to the meaning of those opinions when he himself is involved. In this connection the pretest could point up the areas where the judge should look for morale, but also put him "on the spot" so that he becomes further biased in interpreting the meaning. At least in these results an apparently rather strong inhibiting effect was present.

The major result of this brief and minor study has been to point out that this is a new method which can be of value in providing a broader theory base in the study of human relations training. More extensive and better controlled conditions can result from planning studies which give some indications of the effects of the measures themselves upon the subjects' responses. A great deal of research must be done, however, upon the value of *I* under different conditions, particularly in regard to estimation of significance. It is hoped that this report will serve as a stimulus in this direction.

SUMMARY

This paper discussed the need for and the use of multiple control group experimental designs in one area of current research activity, namely, the study of human relations training. Certain problems in the selection of the control groups were noted, and a brief report of the use of the two–control-group design was presented. Further studies of the use of this interesting and important method were urged.

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THE COMPARISON OF SERIES OF PROPORTIONS: THE PROBLEM AND A NEW SOLUTION

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I. The Problem

Experimental data are often produced in the form of proportions of responses of stronger-weaker calls, yes-no reports or successes and failures to a succession of stimulus magnitudes. The point of an experiment will then usually depend on a comparison of the trend in one series of proportions with that in another. The classic example of data of this sort is, of course, that provided by a series of judgments in which a subject has tried to discriminate the intensity or some other characteristic of the stimulus.

Proper mathematical treatment of such data was a problem studied by Fechner and by psychophysicists for the half-century following him, and the most thoroughgoing solution is usually thought to be the famous process developed by Müller and Urban about fifty years ago. At about the same time came the basically different proposal of Spearman: that of computing the mean of a distribution constituted by the differences between successive proportions. Since that time no basic innovations seem to have been adopted.

The viewpoint of the founders of psychophysics was evidently somewhat different from that of the modern experimenter. The major concern with them was to calculate a set of parameters which would give "the best" description of a set of data. There is also the suspicion that they were sometimes more interested in the problem as a mathematical puzzle rather than as a research tool, and carried the refinements beyond the limits actual data would permit.

From the point of view of the present-day experimenter, the classical methods need re-examination with whatever help can be obtained from modern statistical developments. A process which gives the best description for one purpose may not serve so well for another.

A particular requirement of the present-day experimenter is that he needs to know the sampling error and distributions of the parameters he is comparing. Bruner and Postman (3) have discussed the importance of the error figure in relation to the study of time error in psychophysical judgments. With estimates of the sampling errors the experimenter is able to decide with some confidence whether to accept or reject the null

hypothesis for the differences which appear in the parameters from one situation to another.

II. Previous Proposals

The classical methods were developed at a time when less consideration was given to the need for a probable error.

With the development of general statistical practice at the beginning of the century a number of psychophysicists set themselves the task of deriving an expression for the standard error of the traditional constants, originally in order to decide which method of calculating the parameters is best. Three approaches were used. Urban (12) took as his starting point the deviations of experimental values from the ogive which had been computed as the best fit for them. Thompson (11) started from the standard error of proportions: $\sqrt{pq/N}$. Boring (1) and Culler (4), on the other hand, followed the line of reasoning that had been adopted in common statistical techniques and attempted to derive the standard errors from the standard deviation of the whole distribution. Culler's solutions have evidently been regarded as final, for they are repeated in the standard works on psychophysics and used in some important researches (8, 14). Nevertheless, as will be shown, they do not bear up under examination.

Culler made two attempts at applying the formula

$$SE_M = \frac{SD}{\sqrt{N}}$$

(or its equivalent) to the case in hand. Reasoning chiefly by analogy, Culler attempted to make this transfer in his first paper, then realized that the result had some peculiar implications and decided that the application was incorrect. In his second attempt he used a further analogy to arrive at a much more cumbersome expression. Applying this to an illustrative set of data, he found that it gave nearly the same numerical result as the simpler formula if some, but not too many, of the data were discarded. This leaves the support for the simple first formula very slender indeed, but it is the one which is recommended, without very much reservation, by Guilford (8) and by Woodworth (14).[1]

It is essential, therefore, to look into the validity of both formulas. The Müller-Urban process, of course, can yield a measure of the *SD*

[1] In the summary of his second paper Culler's statement about the simple formula is much stronger than that in the body of the article.

of the curve fitted to the data. If this could be taken as having the same properties as an "observed" *SD*, and if a value of *N* could be obtained, then the conventional formula could be used. Culler's first proposal was that the psychometric curve should be regarded as a cumulative distribution showing the actual number of thresholds below any given stimulus magnitude. It seemed, therefore, that *N* would be the total number of thresholds distributed and hence equal to the number of judgments at one stimulus intensity. (Somewhat similar reasoning had been used earlier by Boring, and rather cautiously, by Brown and Thompson [2].)

This formula has some strange consequences, some of which Culler recognized in his second paper. Since *N* is in no way affected by the number of comparison stimuli used, the experimenter would attain the greatest exactitude in his measurements if he used as few as possible, and concentrated all observations on them. If he makes observations on any other comparison stimuli, he might as well discard them, since according to the formula they will not make his results a bit more precise.

In an attempt to get around this absurdity Culler proposed his second formula. In this, the basic procedure is that *N* should be determined by applying the Müller-Urban weight to each proportion of greater judgments and summing the results. Since no proof is given that this will yield a proper value of *N*, the procedure is hard to criticize. But it does have the following implications:

1. The numerator (*SD*) and denominator (*N*) of the expression for error are made to depend on the same figures.
2. The experimenter would obtain greatest precision in his mean and *SD* if he concentrated all his observations as close as possible to the 50 per cent point, where Müller-Urban weights are greatest.
3. Except in a special case, operations on the judgments "greater" and judgments "less" will give different values for *N*, even with only two categories.
4. If Müller-Urban weights are not used in calculating the median and *SD*, presumably they should not be used in computing *N* for the error formula. Then *N* becomes the total number of judgments for all stimuli, apparently reducing the error, so that computation without Müller-Urban weights gives a more precise value than with them.

Application of the formula to a set of data convinced Culler that a further empirically derived correction was necessary if one wants better than 50 per cent accuracy in the value of the standard error, when the data are not homogeneous. The above deductions strongly suggest that there is something more seriously wrong with the formula.

The meaning of *N* in statistical practice seems to be the number of

independent observations that have been made on some variable. In other words, the experimenter decides on N and observes the value of x that many times. With constant stimuli the situation is otherwise: the experimenter sets a value of x and observes the number of times one of several alternatives occurs. In the first case summing the frequencies in all intervals gives the number of magnitudes observed, and also the number of observations, since each is observed once. In the second case summing will give the number of magnitudes observed but not the number of observations made. Here seems to be the basic difficulty which prevents the analogical use of the "total greater judgments" as N of the error formula.

It is a tempting alternative to identify N of the formula with the total number of reports obtained from O (the implication of Culler's second formula). These are observations of a sort, to be sure, but for N we need "observations of the magnitude of the x variable" (measurements of transition points in this case), and this O's reports are not. Measurement of an x value assigns a certain number to it, with the implication that it lies within the interval that number represents. A single report from O merely locates a transition point as above or below a certain figure—places it, so to speak, in a class with only one limiting boundary. So the total of O's reports cannot be equivalent to the N of the formula.

A recent proposal by Finney (**7**) apparently has a similar starting point, but the basis is the presumed variance of the transformed p-values, rather than the characteristics of the fitted curve. (Finney's paper is primarily concerned with a method of weighting the data and fitting the curve, but only his method for finding error variance is in point here.) To Finney it seems obvious that the variance of the mean transformed ordinate (mean gamma-value) is $1/\Sigma nw$, where n is the number of observations made at each stimulus value, and w the appropriate weight. Likewise, the variance of the slope of the fitted line (the reciprocal of the standard deviation) is said to be $1/\Sigma x^2$, where x is deviation from the mean stimulus. By using these two expressions and the fitted equation, he derives an expression for the variance of the stimulus value corresponding to a 50 per cent division of judgments.

But the two expressions can hardly be correct for the reasons already set forth: there is a confusion in the meaning of "σ" and "n." The implications of these formulas are, therefore, very strange. For the mean gamma-value the variance is said to be determined only by the weighted n's, with no effect of the number of stimuli. Hence the greatest precision would be obtained by concentrating all observations on one stimulus

in the region of greatest w. For the slope, which would be used as a measure of discrimination, the variance is determined entirely by the stimulus values, with the number of observations and S's responses making no difference. The variance is determined before the experiment begins, and an experimenter would do as well to make only one observation per stimulus. Clearly these expressions are faulty.

The other lines of attack on the problem, which actually came earlier, have a more explicit mathematical basis. Urban (12) carried out a derivation based on the deviations from a fitted line, but found results he could hardly believe when he applied it to an actual problem. In spite of criticism by Thompson (11) and Culler (6), at last report he seemed to be standing pat nevertheless (13). (The literature is very controversial. Irwin [9] surveys some of the arguments.) Thompson's own derivation (11) was an attempt to find the errors of the constants from the theoretical errors of the various proportions experimentally observed. The result, when applied to Urban's data, gave a more credible answer differing from the "empirical" figure by a factor of about 2. The Thompson formula is extremely complex, and the calculations it requires are forbidding. (Urban thinks them spoiled by required approximations, though this is not clear.) The formula, it seems, has never been used by anyone, but it needs to be considered as a possibility.

Thompson took it as obvious that the correct expression for the PE of the median or mean (fitted) is

$$w_a^2 = \sum_1^n \left(\frac{da}{dp_k}\right)^2 w_{p_k}^2$$

where

$w_{p_k} = .6745\sqrt{pq/N}$,
a = the median,
p_k = the proportion of "greater" judgments at each stimulus value as shown by the fitted ogive.

The expression in the parentheses is then evaluated by differentiating the "Normal Equation" (used in the Müller-Urban process) which relates a to p. The rationale of the basic equation evidently involves these three ideas:

1. The PE (w_{p_k}) of any point (p_k) on a fitted ogive can be secured by the usual formula for the probable error of a proportion. This, of course, would give a probable error in terms of the ordinate of the ogive and would, if correct, indi-

cate that in the long run half the values of p_k would lie within the limits $p_k+w_{p_k}$, and $p_k-w_{p_k}$.

2. Since it is desired to have the *PE* of the abscissa value a in terms of the abscissa scale, it is supposed that the ordinate *PE* can be converted into a *PE* on the abscissa if it is multiplied by an appropriate value (da/dp).

3. The *PE* of a median based on a series of proportions is taken to be the square root of the sum of the squares of the converted errors of all these proportions.

Now the first of these propositions seems as though it ought to have some proof (the requirement of independence may not be fulfilled), and the other two seem erroneous. In the case of assumption 2, it needs to be remembered that w_{p_k} is not a single proportion but an interval on the scale, containing a range of proportions. For each one included it would seem that a different multiplier would need to be used. If that were done, a different value for w_a would result, and the distribution it represents would be radically changed.

If one looks at Thompson's formula carefully it becomes apparent that an experimenter would not merely waste his time by adding more stimulus values; he would apparently be making his work less and less precise, even though he should increase the number of observations proportionately. Surely it must be that assumption 3 is incorrect. The idea seems to have been transferred from another situation not very closely analogous.

Thus all three attacks on the problem have bogged down. There is even no reason to hope that one or the other of the proposals will give an approximate answer: if we do not know what mark we are shooting at it would be foolish to pretend we have nearly hit it. Actually, an experimenter who followed the implications of any of the proposed formulas would be led to absurdities.

III. A Practical Solution

If one sets the problem in a way slightly different from that of the classical psychophysicists, an easy solution is available. In the "constant process" there is first a transformation of p-values to corresponding gamma values (or *SD* values) by means of the normal probability table. (In the full process they are thereupon weighted.) At this point we may picture a graph on which the points are plotted, the ordinate representing gamma or *SD* values, and the abscissa stimulus values. According to the constant process, then a line of "best fit" is secured by the method of least squares, a line whose mean and *SD* in stimulus terms are taken as the psychophysical parameters. In this application of the method of least squares it is the practice to minimize the devia-

tions (weighted) from the fitted line on the y-axis. For certain purposes this seems a reasonable value to minimize, since the variations in the experiment are primarily in the subject's responses, which yield the gamma values. This process gives a line, it will be noticed, which is identical with one of the well-known regression lines, though the equations given for securing its constants look more elaborate. This regression equation is of the form

$$\bar{y} = r\frac{\sigma_y}{\sigma_x}(x - M_x) + M_y,$$

the form which is appropriate for predicting the most probable y (gamma—*SD*—and eventually p-value) from a particular x or stimulus value. Now the data in hand in an experiment are naturally a set of p-values. So in order to use the regression equation above, it is necessary to turn it around algebraically, making x the value to be calculated from a specified y-value. The intention evidently is to find a stimulus value which will most probably give a certain proportion of judgments in one category if it is presented a great many times.[2]

But one may set out with a different requirement in mind: to find the most probable value of the stimulus which yields a certain proportion of judgments. When the problem is stated so, of course the other regression equation is called for:

$$\bar{x} = r\frac{\sigma_x}{\sigma_y}(Y - M_y) + M_x.$$

This equation is derived by fitting a line as described, but minimizing the squared deviations on x instead of on y. This seems an unreasonable thing to do, since y is undoubtedly the major source of variation. Yet it is the operation called for to find the most probable (or, at any rate, least-squares) value of the stimulus yielding a given proportion of judgments. Using it, one can find the most probable stimulus value for the mean of the gamma-values, and the most probable stimuli for the mean of the gamma-values plus and minus 1 *SD*. It is not necessary to use this equation in reverse; one enters it with a y-value and solves for x. This method of procedure we may call the stimulus regression method.

The numbers produced by the two processes will not be the same;

[2] This use of the regression in reverse makes the expression for the second constant, the mean, very complex. It is derived by first getting an expression for an ordinate value called c, then multiplying it by the *SD* (or dividing by the h) secured from the first equation. The method proposed in the following avoids this complication by using a regression in the normal direction.

they will approach equality as the correlation increases. (Stimulus regression values of *CE* and σ are equal to the conventional values multiplied by r^2.) Since the r in most sets of psychophysical data is very high, the differences between the values will be small. Although one might argue that the new way of framing the problem is better than the old even in theory, the point is scarcely worth debating in most cases because the values will be so close together anyway. (See Table I.)

The great advantage of the second approach is quite plain: the standard error of estimate will provide a measure of sampling error for obtained values. This standard error is derived very simply from r and σ_x, and so avoids the need for deciding what the proper value of N might be. The number of observations will affect the result nevertheless, for as the number of judgments increases, the proportions will be more precisely determined and the value of r will be more exact.

TABLE I

COMPARISON OF PARAMETERS CALCULATED BY "CONSTANT PROCESS" AND STIMULUS REGRESSION METHOD

$S_{p.50}$ = Stimulus when $P = 50$ per cent; Slope (b) = Measure of Dispersion

Source of Data	*Constant Process*		*Stimulus Regression*			
	M	*SD*	$S_{p.50}$	Slope (b)	r	$\sigma_{est.}$
Bressler (from Woodworth) (Without weights)	98.28 grams	7.37	98.32	7.31	.9946	0.59
Bressler (With weights)	98.27 grams	7.22	98.28	7.18	.9980	0.40
Urban *S* 1 (from Brown & Thompson) (Without weights)	99.34 grams	5.30	99.36	5.32	.9988	0.39
Urban *S* 1 (With weights)	99.68 grams	5.20	99.37	5.03	.9800	1.11
Kellogg *S* 1 Table XVIII (Without weights)	3.259 step-units above lowest	1.52	3.249	1.46	.9787	0.411
Kellogg *S* 2 Table XVIII (Without weights)	2.502 step-units above lowest	4.46	2.526	4.25	.9767	0.429

Sampling errors in measurement will have a rather surprising effect on the values obtained by the proposed method. These will obviously be subject to the well-known regression effect. The lower the value of r, the more the predicted values will approach the mean of the stimulus series. Further, errors in measurement will, by attenuation, lower the value of r. Hence, error of measurement will reduce the predicted stimulus values, and a low prediction for SD, for example, would mean either a very good discrimination or very inaccurate measurement. This would be unfortunate, indeed, if it were not possible to distinguish between the two by looking at the standard error of estimate. In other words, comparison of experimental results would always need to be made with reference to the standard errors, which fortunately are available.

The conventionally derived values are not free from such an effect either. The normal equation for SD (in unweighted form) is

$$SD = \frac{N\sum x^2 - (\sum x)^2}{N\sum xy - \sum x \sum y} i$$

where i is the step interval, N = number of stimuli, x = stimulus value, $y = SD$ value. Since the value of x is presumed to be subject to no error, the numerator may be regarded as a fixed value for a particular experiment. The denominator is the familiar sum of the cross-products as found in the numerator of r, as far as the variables are concerned. Random errors of measurement would tend to depress the value of this sum, hence inflate the value of SD, and inflate it in an accelerated fashion. On the other hand, the reciprocal of SD, or the old-fashioned h, which has a normal equation with the fraction inverted, would be depressed by error, in direct proportion to the depression of the corrected sums of squares.

The regression effects for the conventional mean are more complex. Its equation is

$$M = \left(\frac{\sum x \sum xy - \sum x^2 \sum y}{N\sum xy - \sum x \sum y}\right) c + M_{..}$$

The cross-products term in the numerator would somewhat offset the one in the denominator, but the net effect would seem to be an "outward regression."

To interpret a standard error, it is obviously desirable that it should represent a fairly normal distribution. When the stimulus regression method is used, it is assumed that the x-residuals have a normal distribution; it is consistent, therefore, to expect a normal distribution of the

resulting parameters. The Müller-Urban process, on the other hand, in making a least-squares fit assumes that the y-residuals have a normal distribution, or, rather, if weights are used, assumes that underlying p-values are normally distributed. Now if normality exists in one of these three places p-, y (gamma or SD)- or x-values, it cannot be present in the other two. Consider the simplified case in which only two comparison stimuli have been used, yielding the data P_1 and P_2, each subject to a sampling error. The first step in computation would be to transform them to gamma or SD values by means of the normal probability tables. If we think of each of the points as being surrounded by a population, it is clear that the distribution of the population will not be the same after transformation as before. (This is an old observation [11].) Further: suppose we think of the distribution of gamma-SD values about a given point as represented in the third dimension, z, rising from the surface of the paper. There will be two of these distributions corresponding to the two data points. Fitting lines in this case would mean simply connecting pairs of points, and if we connect all possible pairs of points in each layer of the z-distribution, we shall generate a solid in the shape of a molding. If the y-values (gamma-SD) are normally distributed, the molding would display a normal curve when cut by a plane along one of the ordinates. If, on the other hand, the x-values (stimulus) are normally distributed, it would show a normal distribution when cut by a vertical plane on the x-axis. The cut cannot be normal on both axes except in a special case. If one is normal, the other varies according to the slope of the median line, from a rectangular distribution to one more sharply peaked than normal.

So if the p- or y- values are normally distributed, as assumed in the Müller-Urban process, it would be inconsistent to expect the derived parameters, which are x-values, to be normally distributed. As stimulus values seem a more likely place to expect a normal distribution, we have an additional reason for adopting the stimulus regression form of computation. The assumption of normality for p-values is recognized as dubious anyway, since, on the basis of the binomial expansion the sampling distribution of p's is not normal unless p is $\frac{1}{2}$ or N very large. It is, of course, a question to be decided by experimentation whether stimulus parameter values, as secured by the stimulus regression, are normally distributed or not.

It is no small advantage in an s-regression that the parameters are so easily computed. It would be possible to derive a pair of formulas like those of the Müller-Urban process but based on the s-regression. Considering the familiarity of the ordinary regression equation and the ease

with which the necessary elements can be computed, there seems to be no advantage in such additional expressions. The computational procedure would be as follows, using the ordinary regression equation:

1. The p-values derived from an experiment would be converted into SD values by means of the normal probability table.
2. Taking each pair (stimulus value and SD value) as an entry in a correlation, the r would be computed. (The two means and σ's would be by-products of this calculation.)
3. The values of σ, σ_x, σ_y and M_x would be inserted into the x-regression equation, and the equation would be solved for the value of x when $(y - M_y) = 1$. This will give the most probable stimulus for the percentage of judgments 1 SD above the mean of the fitted ogive.
4. The regression equation is then solved for x when $y = 0$. This will produce the most probable value of x for the 50 per cent value of p.
5. The standard error of estimate is then computed by the usual formula. It is the same for both predictions. If, however, the SD is to be considered as an absolute value rather than as a point on a scale, it is the slope of the regression line, and its error is the error of the regression coefficient.

In this method, as in the classical Müller-Urban, weights could be used if the data are exact enough and the need for exactitude very great. With reasonably consistent data, however, experience seems to have shown they are hardly worth the trouble. Most experimenters will probably continue to dispense with them.

The stimulus regression method is not offered as the one and only method of solving the problem, but rather, as a sound practical expedient. Others may be developed which may suit particular purposes better, and it may be possible to develop a procedure which is more general in that it does not require knowledge of the form of the psychometric function or an approximately normal distribution of thresholds on the stimulus scale.

Summary

Since experimenters need to know the sampling errors of the results they obtained, the traditional methods of calculating the psychophysical parameters from percentage data leave much to be desired. The several proposals previously made for calculating the standard errors of these parameters turn out to be incorrect, and it seems the parameters have such distributions that their sampling variation cannot be evaluated.

If, instead, one is content to state the problem as that of finding stimulus values most probably yielding designated proportions of judgments, the problem is quite easily solved. The regression of the transformed proportions on the stimulus scale is calculated, and the standard error is the standard error of estimate.

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BOOK REVIEWS

GIBSON, JAMES J. *The perception of the visual world.* Boston: Houghton Mifflin, 1950. Pp. xiv+242. $4.00.

In these stern days of harsh, quantitative, statisticized psychophysics it is with surprise that one turns up so fresh, so clear, so stimulating, and so unquantitative a phenomenology as is Gibson's scholarly description of what we see and how we see. The book's appearance becomes, in a sense, an event within a tradition: Goethe (brilliant, erudite, dogmatic, wrong); Purkinje (keen, usually right); Hering (argumentative, pedantic); and now Gibson creating the paradox of a phenomenology of vision which is both modern and American.

The account is indeed a phenomenology. There is not a table in this book, nor a graph, nor an equation, nor a quantitative functional relation, nor for that matter a number, except the page numbers, the numbers of the figures, and a few little numbers like *three*, which is the number of dimensions in Euclidean space. The author's critics will note all this and call him unscientific. Very well. Perhaps this is all *Vorwissenschaft* and not science. Who cares about words? The talk is good, penetrating discussion. The argument alters certain basic conceptual views about visual space perception. The discussion asks you—as phenomenology so often does—to become aware of the obvious, to shed many of the hard shells of classical pedantry and to accept instead the author's convincing demonstrations, so many of which are printed right there on the page for you to see.

The eighty-one figures (half-tones and line drawings) are magnificently good. No one can study them without seeing how insistently tridimensional is monocular vision, and perhaps the reader will go all the way with the author (and thus with R. S. Woodworth, too) to decide that the old secondary cues to depth are really the primary ones, and the classical primary cues but secondary.

It is natural and proper for a phenomenology to be descriptive, and Gibson describes, not merely *what* you see, as the Gestalt psychologists do, but also *how* you see. He is analytical. Description has to be analytical. Yet his terms of analysis are not points in a retinal or visual field, or even lines and areas, but relationships, orders. The visual field, he shows you, is ordinal, and it is these topological relationships which are the substance of visual perception. Perceptions in which the ordinal relations are invariant are identical, Gibson notes, using this means for generalizing the various facts of perceptual transposition and of behavioral response to a relationship.

Inevitably the study of perception forces one to face the dilemma of experience vs. knowledge. You look down the long, straight railroad tracks. They are parallel (knowledge) and they converge (experi-

ence). You can *see* the parallels converging. You can see both that the separation does not diminish with distance and also that it does. Faced by this contradiction, modern American functionalism chooses knowledge and lets experience go. If you count on the tracks' being just as far apart away off there as they are here, you will not get in trouble, that is to say, you will be right. Still, you can see them converging. What science takes account of that fact if psychology rejects it?

Of this dilemma Gibson takes a firm grip on both horns. He establishes two correlated systems: the *visual field*, the straight geometry of immediate visualization, and the *visual world*, which consists of the pattern of objects that make up perception. We shall do well to elaborate the specifications of these two systems: the field and the world.

The visual field is *seen*; the visual world is *known*. Thus the concept of the field is the modern descendant of the concept of *sensation*, whereas the concept of the world is descended from the concept of *perception*. In the field parallels may *converge*; in the world they remain always *equidistant*. The field tends to be *areal* and the world *tridimensional*, yet that distinction is not inevitable, for a surface may be an object in the visual world. It is better to say that the field is *pictorial*, representing (but not exactly copying) the *anatomical* pattern of retinal excitation, whereas the world is *objective*, constituted of *ordinal* relations. The visual field is *limited* in extent, is ever *changing* and *fluid*, and is consequently *non-Euclidean*, whereas the visual world is *unbounded*, *stable*, *upright*, *rigid*, and the dear old *Euclidean* space which is what we like to call real. The visual world is thus the *natural* world and the immediate substance of perception. The visual field, which the Wundtians used to think was immediate experience, Gibson believes can be apprehended by a kind of *introspection*, a sophisticated abstraction from objective reality to the system which resembles the retinal excitatory pattern, an abstraction in which artists and introspective psychologists are more adept than others.

When the Gestalt psychologists substituted objects (phenomena, meanings) for the older Wundtian sensations, they were saying that the objective visual world is the primary stuff of experience and that the sensations which introspection yields are the secondary sophisticated artifacts of a special kind of observation. Gibson follows the Gestaltists with respect to the objective visual world: objects are what human adults, children, and at least the higher vertebrates have in their perceptions ("size constancy" holds for chickens). So the one school inverts the other. For the Wundtians, sensory experience is primary and objects are generated from it. For the Gestaltists, objects are the stuff of experience and primary, with sensations becoming psychologists' constructs abstracted from the objects. Gibson's view of the relation of the field to the world is not clear to the reviewer. He seems to think of the field as primitive and the world as primary, of the field as genetically

prior to the world and of the world as temporally prior to the field in observation. You cannot be quite sure whether the revelation of the field to introspection is an atavistic process or an act of sophistication.

The greater part of Gibson's book is the description of the way in which the visual world of objects depends upon the visual field, while differing in so many of its properties. The basic demonstration shows how the tridimensionality of the visual world is the consequence of certain ordinal relations in the visual field, relations of which one of the most important and most novel is the *gradient* of *texture*. Gibson constantly stresses gradients as conditions of objective perceptions because gradients show how the ordinal properties of an entire field provide the essential basis for the spatial orientation of perceived objects and for the organization of the objective world as a whole. Texture is large grain or macrostructure, tiles in a pavement, clumps of grass or soil in a field, stones in a gravel pit, anything that is constantly repeated in units of about the same real size. Just as the railroad tracks converge in the distance of the visual field, so the grain of the ground gets more finely structured in the distance, and the gradient of change in texture tells you, not only how fast the ground spreads away from you, but also how far below you it is.

Gibson's epistemology is correlational—like Mach's, or, more recently, B. F. Skinner's empty organism. What Gibson calls a "theory" is thus only a description of a correlation, a theory which tells *how* but skimps on *why*. That is, of course, all right for phenomenology, provided the enthusiastic describer does not forget that eventually science must go deeper into the means of correlation, must show in physiology *why* a gradient of texture produces a perceived depth, not merely that it does.

The social psychologists will be piqued to note how well Gibson gets along without using the concept of set, attitude, motive. He has, nevertheless, to come to the dynamics of perception in the end, as he does in his chapters on meaning and learning, and this reviewer thinks he would have fared better had he not resisted the inevitable so long.

There ought to be some accepted device by which anachronisms could be avoided in bibliographies. In this respect Gibson is no better than others. He gives the date of a translation but not the date when the thought was first published. When he has used a later edition he gives its date, which makes his page reference accurate and misleads the student as to when some thought was averred. Thus the careful student who wonders enough about history to examine dates in the bibliography will be misinformed about Helmholtz, Mach, LeConte, Berkeley (who "wrote" for Everyman in 1922!), and a few others.

It is too bad that the reproduction of the text of this book is inferior to its splendid figures. The verbal matter is the offset reproduction of typing. The spacing is poor, much poorer than is required by this method.

It is interesting to see phenomenology asserting a role in modern American psychology. To some extent Gibson rides the Zeitgeist. Retinal disparity and convergence he relegates to secondary positions, and for accommodation he has little use. If, however, the Zeitgeist is headed for the behavioristics of operationism and for a monistic unity of science, then he lags behind progress. He should have removed all doubt about the distinction between the visual field and the visual world by describing the operations by which each can be observed in chimpanzees. That would be, however, a good deal to ask of a phenomenology, which has a right to be just what it claims to be and not something else; and, moreover, how can we be sure that the Zeitgeist is set eventually to make behavioristics the indisputable scientific truth? All in all the reviewer finds this book a remarkably keen, clear, and wise description of just how it is that people see things. The chimpanzee phenomenology of vision, moreover, may need to wait until we have trained a chimpanzee to write it.

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HARRIS, R. E., MILLER, J. G., MUENCH, G. A., STONE, L. J., TEUBER, H. L., & ZUBIN, J. *Recent advances in diagnostic psychological testing.* Springfield, Ill. Charles C Thomas, 1950. Pp. x+120. $2.75.

This symposium of six contributions opens with an introductory essay by Miller on contemporary trends in psychodiagnosis and proceeds through contributions by Harris on psychodiagnostic testing in psychiatry and psychosomatic medicine, by Teuber on testing in neuropsychology, by Muench on the uses of diagnostic psychological methods in counseling and psychotherapy (including their validation), by Stone on recent developments in diagnostic testing of children, and a final chapter by Zubin on test construction and methodology. The book is even more brief than its 120 pages might indicate since 15 of them are taken up by bibliography, the pages themselves are small, and the type face is reasonably large. It would, perhaps, have been better presented as a monograph. The contributions are excellent, however, and they are well integrated, with a consequent absence of annoying overlap.

Miller's introduction is competent and timely; Muench's contribution deals interestingly with a not too often discussed facet of testing; and the Teuber and Zubin articles are able and stimulating performances in special technical fields. In this reviewer's opinion, special praise should go to the papers by Harris and Stone, where demonstrated clinical sensitivity goes hand in hand with theoretical competence and thorough appreciation of experimental methodology. They are a pleasing confirmation that the psychologist can function clinically

without sacrificing his scientific heritage and that the blending of these two influences can be a promising and possible reality, not a mere ideal fiction kept alive by the stereotypes of our present professional training programs.

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WORTIS, JOSEPH. *Soviet psychiatry.* Baltimore: Williams & Wilkins, 1950. Pp. xv+314. $5.00.

Outside the borders of the U.S.S.R. knowledge of Soviet psychiatry and psychiatric practice has been excessively meager. Dr. Wortis in his book *Soviet Psychiatry* has tried to reduce this lack by presenting, against an historical and ideological background, a report and modest analysis of the contemporary state of the Soviet psychiatric scene. The book covers a considerable range of material, but fails to produce the kind of account we should have liked.

There are a number of basic reasons for this failure. They can, for the most part, be summarized in the author's uncritical relationship to the language and content of his material which, even if adequate in coverage (and it is not), would be insufficient basis for the task the author set for himself. The realities of the U.S.S.R. are not to be plucked directly from a literature which is commanded to eschew "objectivism" and to exhibit the requisite "partisanship" demanded by the Party. Because the Soviets speak of "free, creative discussions" does not mean that there *are* free, creative discussions; nor must we necessarily acknowledge the Pavlovian basis of contemporary psychiatry in the Soviet Union, merely because the "glories of Pavlovian theory" are dutifully intoned in the prefaces of books and the columns of the press.

Yet the author of *Soviet Psychiatry* does precisely this. The declarative allegiance to Pavlovian theory in much of Soviet psychiatry is not perceived as such, and the model, nonexistent "Soviet psychologist" and "Soviet psychiatrist" are made the uniform spokesmen of the living flesh and blood. Thus, from the author's account one would not gather that Pavlovian theory was ever termed in the U.S.S.R. a "verbal husk" and a "huge mechanistic peril" to Soviet psychiatry; nor would one have suspected from his account that there could be ground for saying, as Ivanov-Smolenskiĭ did in a meeting called in 1950 to introduce *in fact* "Pavlovianism" into Soviet psychiatry, that "until recently all [real] attempts at application of Pavlovian theory to the problems of psychiatry have been bayoneted invariably."

This book is further marred by unnecessary excursions into Soviet apologetics. "Forced labor camps" are said to demonstrate the Soviets' "deep confidence in the wholesome restorative functions of work." And

the disgraceful Lysenko affair, whose gratuitous introduction into the book serves no useful purpose, is unrealistically excused away.

One could refer to other serious defects, such as the failure on the author's part to understand that references, distributed over a period of years, cannot be amalgamated at will to yield a composite contemporary picture. However, with the proper reservations in mind, the circumspect reader will find much of interest and value in this book on a subject little explored outside the U.S.S.R. The book is a pioneer attempt in a field where the pitfalls are many. For this reason alone the author is to be commended for venturing on so difficult an assignment. However, the book which can be said to have presented Soviet psychiatry and psychiatric practice adequately still remains to be written.

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VON FRISCH, KARL. *Bees: Their vision, chemical senses, and language.* Ithaca: Cornell Univ. Press, 1950. Pp. xiii+119. $3.00.

As indicated in its title, this thin volume (119 pages) describes the visual and chemical senses of the honeybee. The evidence, much of it gathered by von Frisch over the past thirty-five years, is presented clearly and interestingly in nontechnical language.

The bee's color sense varies from that of human beings. Ultraviolet radiations are visible to this insect, but it is not sensitive to red. Form perception depends upon the "brokenness" of a pattern, and this may be related to the fact that bees use vision primarily while in flight. Von Frisch suggests that "broken" patterns produce a flickering sensation. The olfactory sense of the honeybee is closely comparable to that of a man "whose sense of smell is very well developed." Bees differentiate between sweet, salty, sour, and bitter tastes but are much less sensitive to bitter substances than are human beings.

The most interesting part of the book is that which describes what the author prefers to call "the language of the bees." As a result of many years of careful, ingenious investigation von Frisch is able to present evidence of which he says, "No competent scientist *ought* to believe these things on first hearing." Those who were fortunate enough to hear Dr. von Frisch lecture during his visit to this country in 1949 may have shared with this reviewer the conversion from skepticism to enthusiastic acceptance of von Frisch's published findings.

The gist of his reports is as follows. When a honeybee finds a new source of nectar and returns laden to the hive, she behaves in such a way that her hive mates leave the hive and fly to the location of the food source. The bees are influenced by chemical stimuli associated with the "discoverer" and also by the "dance" which she performs on the vertical surface of the comb. The pattern and tempo of her dance vary according

to the location of the food supply and also according to the distance that must be traversed to reach it.

Von Frisch has found that the pattern of the dance is controlled by the plane of polarization of diffused sunlight. The degree of polarization in any portion of the sky at a given time is a function of the sun's position and the structure of the bee's compound eye, particularly the structure of the single ommatidium which appears adapted to function as an analyzer of polarized light. The direction in which certain portions of the dance are oriented varies according to the direction of the flight path to the food. The speed with which parts of the dance are performed is inversely related to the distance flown.

By reacting appropriately to the cues mentioned above, bees that have never visited a particular food source are enabled to proceed directly from the hive to the new supply of nourishment. In what appear to be well-controlled experiments bees were observed to fly as far as 6 kilometers (3.7 miles) straight to a new feeding place from which a hive mate had recently returned loaded with nectar.

This book can be strongly recommended to readers who are scientifically concerned with comparative psychology. But it will also prove entertaining and instructive to laymen who are interested in natural history. The literary style is simple and charming. The subject matter stands as a model of patient, careful investigation of complex patterns of animal behavior.

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BROWN, WARNER, AND GILHOUSEN, H. C. *College psychology.* New York: Prentice-Hall, 1950. Pp. xviii+507. $3.75.

The character of this text is set in two remarks by the authors in the early chapters of the book. "Psychology," they write, "is the study of behavior and experience." A bit later they observe, "So, instead of presenting theories, we continue in the next three chapters, as we began, maintaining an externally descriptive attitude." This last applies to the treatment of learning in particular, but could just as well apply to the whole work. Reading these, one would expect a book which is eclectic, nontheoretical, and rather traditional, and so it is.

The bifurcate definition, of course, enables the authors to stand with one foot on each of the familiar foundations: reflexology and the analysis of consciousness. In practice, indeed, the body of doctrine becomes quadripedal, for in the last fourth of the book, the authors write a psychology in terms of adjustment, then a psychology in terms of abilities. By and large, the authors seem to find the relations between the four different limbs too complex and uncertain to discuss in such a book.

The "descriptive attitude" the authors favor means, no doubt, predilection for facts and a postponement of theory. One can heartily

praise an eclecticism of facts, but a jumbling of theories is not so clearly admirable. Granted that it could be done at all, the exposition of facts picked clean of theory would be a most difficult operation, and the result not likely to be palatable to first-year students. So it must be expected that authors contemning theory will, nevertheless, present their facts embedded in theories—diverse and sometimes very aged theories taken unwittingly from the general heritage.

For example, when the authors write, "what we ourselves contribute to the operation of thinking . . . consists of the ideas, memories, concepts, and images that 'pass through our minds,' " they are hardly reporting a simple untrammeled observation but, rather, repeating a very old theory. So, in "attending" (which is the "selective process" and the "awareness of concentration" "prior to perceiving") "the self," "adjustment," "motivation," etc., there is evident a matrix now of one psychological theory, now of another. As one goes through the book these are found intermingled. Simple learning is a matter of conditioned response. In trial-and-error learning there is activity ("trying to accomplish something"); in problem solving there are in addition "ideas," and thinking is wholly a matter of "ideas." If psychological theories are a vice, as Brown and Gilhousen maintain, "because they obscure the important distinction between facts that are actually known and a fanciful veneer of inventions covering the enormous area of the unknown," they have, indeed, taken a strange path to virtue by intermingling more than the usual number of different theories.

In the introduction, the authors announce that they do not expect the student to weigh or interpret experimental evidence but will cite experiments, when necessary, to make their statements concrete, choosing studies as old as possible so as to impress upon the student "the truth that psychology has historical foundations." It is clear that they themselves have great respect for psychology with historical foundations, and they would probably be well pleased to note the resemblances of their book to those written thirty years ago.

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WIENER, NORBERT. *The human use of human beings.* New York: Houghton Mifflin, 1950. Pp. 241. $3.00.

The author of *Cybernetics* has written a related book for the layman to emphasize the "social consequences of his point of view." The issues treated can scarcely be traced to a single point of view, but they are an interesting and vigorous lot. There are discussions of the direction and nature of human progress, the distinction between rigid instinctive behavior and modifiability, man's exclusive claim to speech, the nature of the individual, the paradoxes of the law of patents, the evils of scientific secrecy, the education of the scientist, the second industrial revolution,

the Catholic Church as a totalitarian institution, and so on. Although "thinking machines" are frequently mentioned, and many parallels are drawn between them and living organisms, few if any of the author's conclusions are derived from cybernetics itself in any rigorous way. It is a book of strong, often violent, opinion, ranging over many of the fields which an intellectual with compelling political and moral persuasions might be expected to touch upon in the course of an active life. Its manner swings from the stiffly academic to the witty, from the dogmatic to the humble, from the exaggerations of science fiction to the caution of the trained scientist.

Psychologists will not find much that contributes to their science. The author has brushed lightly against physiology, particularly the works of Sherrington and Cannon, but whenever his comparison of thinking machines and living organisms becomes in any sense concrete, the assumed mode of operation of the nervous system has a shopworn look. His acquaintance with behavioral processes is even less extensive. For example, although proprioceptive feed-back from behavior is one of the cardinal issues in *Cybernetics*, there is only the most superficial account of what such a feed-back is like, and no discussion whatsoever of experimental work in the study of learning in which the effect of feed-back upon behavior is analyzed.

In spite of this, the book has a certain value in reminding us that scientific achievements are seldom without an impact upon civilization. The special impact with which Professor Wiener is here occupied has certainly not received the attention it deserves.

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